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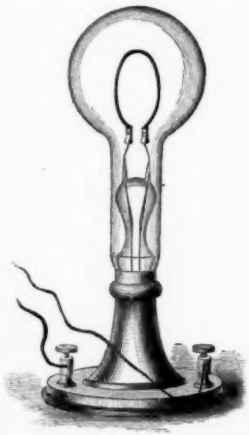
MEASUREMENTS OF AN INCANDESCENT PAPER-CARBON HORSE-SHOE LAMP, CONSTRUCTED BY MR. T. A. EDISON.

By HENRY MORTON, Ph. D.; A. M. MAYER, Ph. D.; and B. F. THOMAS, A. B.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

It may seem almost superfluous to describe the carbon horse-shoe electric lamp as recently constructed by Mr. T. A. Edison, so much has been written about it in journals of all descriptions, from the daily papers upwards; but to



make our work complete we will state briefly that the lamp measured by us, and represented in the accompanying cut, consists of a pear-shaped glass globe, with two re-entering tubes at its smaller end, through which are passed platinum wires, with little screw clamps at their upper ends, which hold the ends of the carbon horse-shoe.

This horse-shoe is 1.18 inches high, and 0.72 inches across at the widest part.

It is made by charring a piece of thin card-board of similar shape, out of contact with air. The interior of the globe is very perfectly exhausted. Fine copper wires connect the platinum wires with the binding screws on the wooden base of the lamp.

The present writers believe that the following measurements made by them, in the Physical Laboratory of the Stevens Institute of Technology, will possess some general interest as being the first full and accurate series of determinations, giving the fundamental properties of one of these instruments.

The lamp in question was one of the paper horse-shoe style, No. 154, given by Mr. Edison to the editors of the *Scientific American*, and by them kindly loaned to us.

We have failed to obtain other lamps directly from Mr. Edison, seemingly because of the offence taken at Menlo Park to the emphatic contradiction which one of us thought it right to give, at the very outset, to the unfounded claims for Mr. Edison's lamp which were then published by some of the daily papers.

The lamp here described is certainly a fair specimen of the type to which it belongs, as appears from a general comparison of results with those obtained by the scientific men who recently measured a number of these lamps at Menlo Park under the auspices of Mr. Edison himself.

The work herein described has been in progress for nearly two months, being frequently interrupted by the pressure of other engagements.

The experiments naturally divide themselves into three groups. I. Determination of resistance of lamp at different temperatures, as indicated by luminous power and by energy absorbed. II. Determination of average of light given out by a lamp in all azimuths. III. Determination of current-strength in circuit, corresponding to various intensities of luminous power of lamp and deflections of galvanometer.

With these data the determination of relation of luminous power to energy, expended in the lamp itself in producing the same, was a matter of direct calculation.

I. Determination of the resistance of the carbon loop of lamp at different temperatures, as indicated by luminous power of the same.

A preliminary experiment having shown that between 50 and 60 cells of a Grove battery, with active zinc surface of 20 square inches and platinum surface of 18 square inches in each cell, was required to develop the requisite electric current, such a battery was set up and connected piecemeal with the rest of the apparatus arranged as follows:

The battery current was divided into two branches, which traversed in opposite directions the two equal coils of a differential galvanometer having .33 ohms resistance in each coil. One branch then traversed the lamp which was placed in a Bunsen photometer, made by Sugg of London. The other branch passed through a series of adjustable resistances composed of German silver wire, stretched in the free air of the laboratory, to avoid heating. (Careful tests showed that this precaution fully accomplished the desired result.) The united branches were then carried to the other pole of the battery.

These arrangements having been made, a certain number of battery cells were put in circuit, and the resistances adjusted until the galvanometer showed no deflection. The condition of the loop was then observed in perfect darkness, and when its light was measurable it was taken by varying the distances of both lamp and candle as circumstances required.

Thus, for the lowest candle power taken, the lamp was at 15.8 ins. from the photometer, and the candle at 50 inches.

The results so obtained were as follows:

No. of cells in circuit.	Candle power.	Resistance.
0	0	123 Ohms.
5	0	113.5 "
10	dark red	106. "
20	.1 candle	94. "
25	.2 "	89. "
30	.4 "	87. "
35	.9 "	83.7 "
40	1.9 "	82. "
45	5.1 "	79.8 "
50	8.4 "	78. "
58	18. "	75. "

These results are also expressed in the curve shown in Diagram 1.

The fact of a decrease of resistance, with rise in temperature with carbon, was previously noticed by Matthiessen in 1858. (See *Phil. Mag.*, Vol. XVI. pp. 220–221.) This experimenter found the electric conductivity of ordinary gas coke to rise about 12 per cent. between the common temperature and a light red heat.

In the case of this delicate thread of impure carbon constituting the loop of the lamp, the rate of increase in conductivity or fall in resistance is more rapid.

Diagram 1 shows the above observations plotted as a curve, and needs no further explanation.

In the above discussion we have compared the resistance of the lamps with the luminous emissions only; but we have also considered it worth while to make an analogous, but more extended comparison, namely, one between the resistance and the total heat, or total heat and light generated in the lamp. This enables us to carry the range of comparison below those points at which sensible light is developed. As a matter of course this relation to total heat is also the relation to energy transformed

Diagram 1.

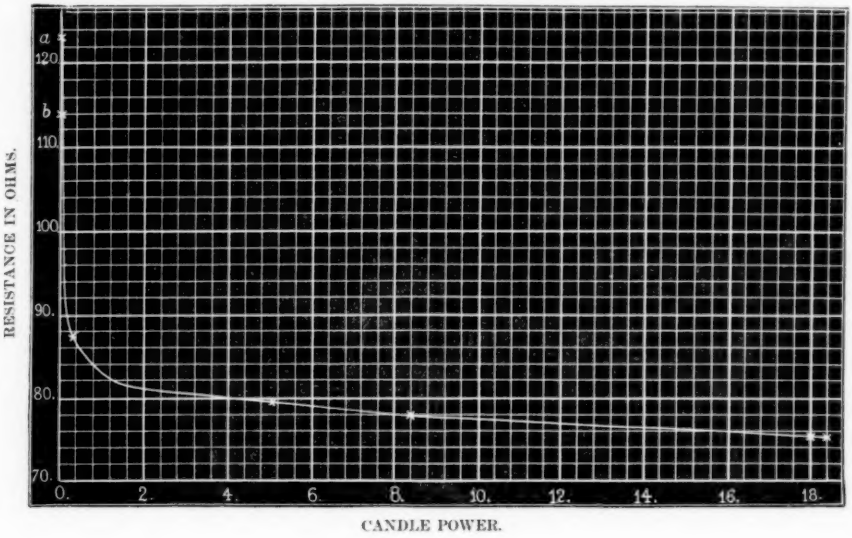
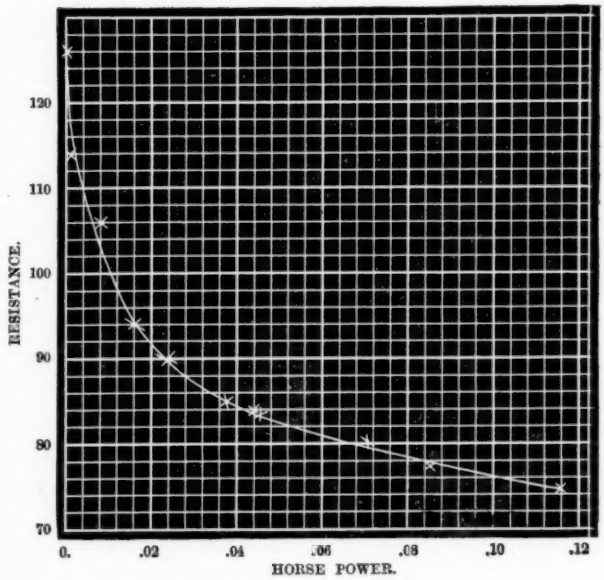


Diagram 2.



and this also we have given in the following table expressed in foot pounds and in horse power.

These results are also expressed in the curve Diagram 2, and it is interesting to notice the general similarity of this curve with that of Diagram 1.

It might at first seem desirable to establish a temperature ratio in the same connection, but when we reflect that this would depend on a number of conditions liable to variation with individual lamps, and would really have no practical bearing on the question of power consumed and light produced, it will be seen that this line of investigation hardly promised enough to warrant us in pursuing it. For example, if the carbon loop were surrounded by a less perfect vacuum, or by one or another gas, such as nitrogen or hydrogen, great differences in temperature would no doubt be found even with the same resistance and current, or total heat.

Energy transformed into heat and into heat and light in loop of lamp.			Resistance in Ohms.	Candle Power.
As foot pounds.	As horse power.	As total heat Units.		
66	.002	.0855	114.	0
83	.0025	.1069	112.	just vis.
122	.004	.1710	111.	dull red.
244	.008	.342	106.	cherry "
488	.016	.684	94.	.016
792	.024	1.026	90.	.10
1254	.038	1.624	84.4	.59
1452	.044	1.881	84.	.83
1518	.046	1.966	83.3	1.10
1650	.050	2.137	82.5	1.5
2343	.071	3.035	80.	4.5
2838	.086	3.676	77.6	9.2
3828	.116	4.958	74.5	20.0

II. *Determination of the average light of the lamp in all azimuths.*—It was noticed at once that there was a vast difference between the amount of light given out by the lamp in a direction transverse to the plane of the loop and in the direction of that plane, the former quantity being about three times as great as the latter, and while it would of course be possible on certain assumptions to estimate what should be the average, it was also perceived that a direct determination by experiment would be far more reliable and important than any amount of theoretical discussion.

The lamp was therefore mounted on a divided circle with the axis of the lamp passing through its center, which rotated. A fixed index measured the angle of rotation of the circle and lamp, and was so placed that it marked zero when the plane of the horse-shoe was in the axis of the photometer.

This position of zero was indicated by a well defined line of shadow of the nearer half, thrown by the further half of the loop on the photometer disk.

The lamp was rotated 10° at a time, and several readings were made in each position, the averages of which are given in the accompanying table.

This shows the results for one quadrant. Similar experiments were made for three other quadrants with like results. Diagram 3 exhibits the results of the table plotted in a curve.

Candle power of Loop in various azimuths.

Angle of plane of loop to axis of photometer.	Candle power.
0°	6.7
10°	6 9
20°	8.4
30°	12.8
40°	14.3
50°	16.3
60°	17.7
70°	19.1
80°	19.8
90°	20.6
	10)142.6

Average=14.26=69% of Maximum.

III. *Determination of current strength in circuit, corresponding to various intensities of luminous power in lamp and of deflections of galvanometer.*

For these determinations the apparatus was arranged as follows:

The battery current was passed through one coil of a Gaugain galvanometer, then through a copper voltameter, and then through the lamp placed in the photometer, thence returning to battery.

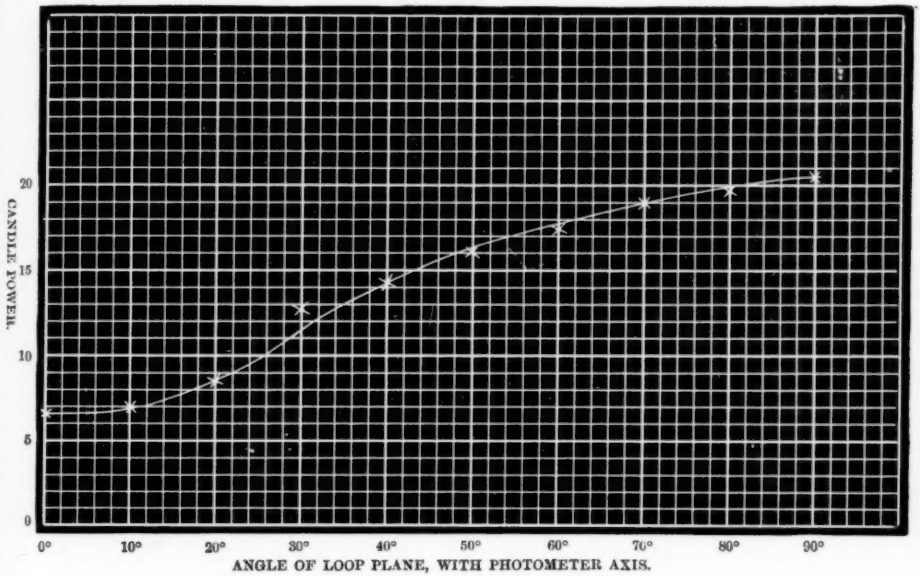
The amount of copper deposited in a known time gave of course the current strength, in view of the fact that a current of one weber deposits .326 milligrammes of copper in a second.

Thus in the first experiment 1062.4 milligrammes were deposited in an hour, or in 3600 seconds; therefore $\frac{1062.4}{.326 \times 3600} = .905$ webers current.

Three experiments were made of this sort, the data and results of which are given in the following table:

Weights of cathode.			Time minutes.	Current webers	Max. candle power.
Before.	After.	Gain.			
43398.4	44460.8	1062.4	60	.905	15.
48314.	49110.	796.	40	1.017	17.6
43105.	43617.	512.	25	1.047	19.8

Diagram 3.



From the current in webers and the corresponding resistance in ohms, where these had been determined as above, it was of course easy to deduce the exact amount of energy transformed into light and heat, and to compare the same with the actual candle power afforded by the lamp at the same time.*

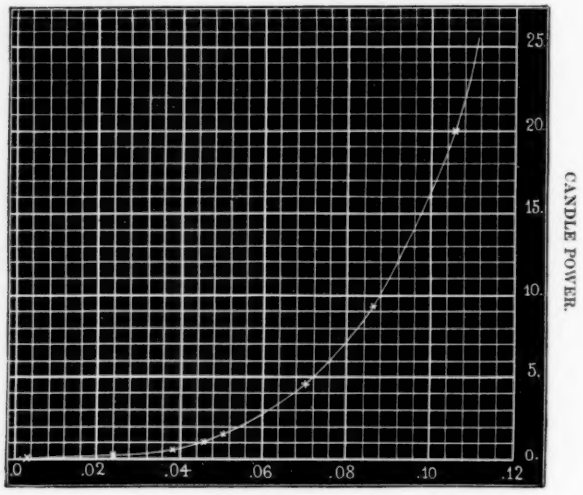
To make the results more general, however, the constant for the tangent galvanometer, used in all the experiments, was determined, so that the current strength corresponding to its readings could be obtained in the cases where the voltameter had not been employed. This constant was found to be .262, so that the tangent of the galvanometer readings multiplied by .262 gave the current strengths in all cases.

* NOTE.—Thus, for example, in one experiment the average candle power being 10 candles, the resistance of the lamp was 76 ohms, and the current .905 webers.

IV.—*Determination of power consumed by lamp alone in maintaining light of different intensities.*

No. of cells of battery.	Deflection of galvanometer.	Current in webers pr. sec.	Resistance of lamp.	Horse power.	Candle power. Maximum.	Average.
8	23.°	.111	114	.002	.0	
9	26 2	.129	112	.0025	just visible	
10	32.	.164	111	.004	dull red	
15	42.4	.239	106	.008	cherry red	
20	54.	.361	94	.016	.016	
25	59.5	.445	90	024	.10	.07
30	65 6	.578	84.4	.038	.59	.41
33	67.3	.626	84.	.044	.83	58
34	67 9	.645	83.3	.046	1.10	.77
35	68.7	.672	82.5	.050	1.5	1.05
40	72 1	.811	80	.071	4.5	3.1
45	73.9	.908	77.6	.086	9.2	6.4
50	76.3	1.079	74.5	.116	20.0	14.

Diagram 4.



As regards the economic relations of this subject, it will be interesting to notice that an average light of 14 candles being obtained at the expense of 0.116 of a horse-power in electric current, each horse-power of electric energy would furnish 120-candle power in these lamps. To obtain this horse power of electric energy, however, considerably more mechanical energy must be applied to the driving pulley of the electric generator. If the loss so encountered was 40 per cent., as appears to be the case with some of the best machines which have been measured accurately, this would reduce the light developed to 72-candle power for each horse-power of mechanical energy applied to the driving pulley of the electric generator.

When we remember that with the arc light there has been obtained from 1,200 to 1,800 candle-power per horse-power of mechanical energy applied to the generator, it is evident that Mr. Edison's lamp, as now made, does not escape the enormous loss which has heretofore been encountered by all forms of incandescent electric lamps.

DOMESTIC MOTORS.

Translated from Revue Scientifique.

One of the most interesting communications to the convention of civil engineers was certainly that upon domestic motors by M. Fontaine, chief editor of the *Revue Industrielle*. We present here the general principles only, omitting the special engineering details.

M. Fontaine set forth at once the advantages of small motors. "It is a reproach against the steam engine that it has brought about a centralization in manufacturing, a result fatal to the advancement of the working classes." A division of the motive power would permit work at the fireside, work in the family. For certain kinds of work, such as that of the common sewing machine, the utility of a small motor need not be demonstrated.

The author then passed in review the different kinds of motors; engines driven by springs, electric engines, hydraulic engines, steam and gas engines.

Engines driven by springs are not

motors in the ordinary sense, as they develop no work by themselves. They have only the property of storing a small amount of motive force developed by muscular action, and of releasing it under conditions entirely different from those attending its accumulation. Thus by turning a crank slowly with considerable effort for a short time, we store up a certain amount of work which may be made to run a light machine at high speed against a light resistance, through a comparatively long time. Unfortunately only a small part of the work is utilized, and the labor of winding up the machine is far from being compensated by the useful effect obtained. The amount of work that can be accumulated in a steel spring without passing the limit of elasticity is, of course, limited. It will vary naturally with the quality and size of the spring. Experience shows that when employing the best steel known, converted to the form of a clock spring (which is the most favorable for such a service), the amount of work stored will not exceed 40 kilogrammeters for each kilogram of metal.

In practice the loss from friction and deformation of the spring is about 80 per cent. It is true that the majority of the steam engines afford no better return of the total work stored in the fuel, but these latter consume coal only, while the spring motors run at an expense of muscular force, which is the most expensive and the most precious of all sources of mechanical work.

M. Fontaine declared that the springs could be profitably replaced by a weight which would restore a large proportion of the labor expended upon raising it. A weight of 100 kilograms raised three meters would afford a very economical accumulator, and one less liable to deterioration.

From electric engines we can no longer hope for economical results. An electric motor is nothing more than a reversible magneto-electric machine. The latter will receive a current of electricity, and under its influence will take on a rotatory movement. But from whence comes the current? From a galvanic battery or from another magneto-electric machine? In the latter case it is only a transmission of power which is effected by electricity, and it is not a solution of the problem of domestic motors. If the power is derived from a battery, then the battery is consuming zinc and acids, of which the price per unit of work is far above that of coal or gas, or any of the so-called combustibles.

It suffices to know that a magneto-electric machine, worked by a single man, develops as much electricity as a battery of six Bunsen cups, each eight inches high, and freshly charged, in order to conclude that it would be necessary to employ more than six cups to drive a machine of one-man power.

Engines driven by water seem much more attractive. They require no fuel nor any special agent to operate them. But while the fear of fire is not attendant upon their use, the accidents arising from freezing have their inconveniences.

But here again it is the question of economy, which is of the first importance; and as the cost of water varies much in different places, M. Fontaine bases his calculations upon the conditions which obtain in Paris. This city possesses, on the one hand, a good supply of water, and on the other, supports a multitude of industries based upon indoor labor at home.

The pressure of water in Paris is equal to a head of 40 meters in the neighborhood of the Seine, and only 10 meters in the higher portions. In more than half the dwellings the water cannot be delivered in the upper stories.

The charge for water from the Dhuys or the Seine is 0.f33 (6¼ cents) per cubic meter, if the quantity used is not more than 5 cubic meters per day. When from 5 to 10 meters are used per day the price is 0.f27, and for 10 to 20 cubic meters it is only 0.f22. The water of the Ourcq costs one-half less, but the pressure is too slight to be serviceable for motive power.

Assuming a pressure corresponding to a head of 20 meters and an efficiency of sixty per cent. for the motor, the quantity of water necessary to afford a work equal to six kilogrammeters per second will be 1,800 liters per hour and 18 cubic meters in 10 hours. The daily expense would be four francs.

There are many cities, however, in which the pressure is high and the price moderate. In Lille, for instance, the head is 30 meters and the price only

seven centimes (1⅓ cents) per cubic meter. In Switzerland most of the cities are provided with a water supply.

After enumerating and describing the various forms of hydraulic motors, M. Fontaine passed to the consideration of small steam engines. There are in reality but few domestic motors of this class, and M. Fontaine has best explained the reason for this fact by re-counting the revolutions through which a little engine of his own invention was made to pass. The authorities would not permit his microscopic boiler to be used without the usual safety apparatus; valves, gauge-cocks, water level indicators and all, and this was notwithstanding the fact that the boiler could not be fed while working, and the limit of pressure was provided for.

The necessity of obeying the ordinance of 1865 prevented this engine from giving good results, because the safety apparatus, reduced to the scale of the boiler, worked so badly.

We come finally to the gas engines. These are the most numerous and certainly best motors for light work, provided a supply of gas is available.

To define a gas motor it will suffice to quote the words of M. Armengaud Jr., in a lecture on the subject before the conference at the Trocadero in August 1878 (see VAN NOSTRAND'S MAGAZINE, Vol. 20, page 148).

"A gas engine possesses the essential organs of the steam engine; the cylinder which receives the gaseous fluid; the piston, which, by aid of rod and crank, transmits the pressure to the shaft; the fly-wheel, which regulates the motion, and the pulleys and belts by which the power is conveyed to the machines to be driven.

"The gaseous fluid is a mixture of gas and air, in such proportions as is most susceptible to explosion when in the neighborhood of an ignited body. The mixture is exploded by a little flame, and the products of the combustion suddenly dilated by the heat, urge the piston, and thus develop the motive power."

The Hugon engine was one of the first to prove capable of application to industrial purposes. It utilizes the expansion directly. It is a double acting engine, and the piston at each stroke admits the explosive mixture (which is exploded at the half-stroke) on one side, and expels the products of the previous explosion on the other. The mixture is exploded by a gas flame. The engine is horizontal.

The Otto and Langen engine is vertical, and employs the force of explosion indirectly. The piston is first driven upward like a projectile by the direct force of the explosion. The gases expand, following the piston; and even after they have become reduced to the atmospheric pressure, the piston continues on its upward stroke by reason of its acquired momentum. It stops when the atmospheric pressure has absorbed the accumulated work. The gas under the piston has become rarefied and cooled; the watery vapor is consequently condensed and the piston descends, urged by atmospheric pressure aided by its own weight.

This mode of action which has proved very economical when compared with other methods, has the unfortunate peculiarity of making an insupportable noise, and which has, in great measure, prevented its extended use.

The Bischopp engine belongs also to the class which utilizes the explosion during the ascent of the piston. The cylinder is vertical, and the piston communicates its motion to the shaft by means of a connecting rod. These machines have been constructed for light work, and especially to drive sewing machines. They are run in Paris at an expense of 10 centimes per hour for $\frac{1}{15}$ of a horse power, or of 25 centimes for $\frac{1}{2}$ a horse power.

In 1877 a Bischopp motor attached to an electro-plating establishment ran without any attention 47 days and 47 nights; that is, until it completed the work. No other known motor could have done this.

"Our conclusion," says M. Fontaine, "is plainly derived from the results of such examinations as these.

"In the present state of our knowl edge, we would advise buyers to get the small Bischopp gas engine, and would advise inventors to seek to devise a small steam engine to be run by burning coal, and furnished with an automatic regulator for the combustion."

BALANCING LOCOMOTIVE ENGINES.

By C. A. SMITH, B. S., Fort Wayne, Ind.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THERE is comparatively little information to be found in print upon the subject of balancing engines, and in works where the subject has been touched upon at all, the information given is so brief and condensed that little satisfaction is obtained by mechanics who may be seeking for knowledge in this direction; and as the writer has been frequently consulted, he hopes to be able to give such information in the following pages as may be desired by those interested.

The importance of balancing locomotive engines will be questioned by no one who has had sufficient experience in machinery, if not a technical knowledge of the same. It prevents unnecessary "wear and tear" of the machinery as well as dangerous oscillations which would have a tendency to cause the engine to jump the track. As the forces which cause these oscillations increase as the *square* of the angular velocity of the wheels, other things being equal, it becomes very important to take particular pains in balancing high speed or passenger engines; this not only to secure safety to life and property, but also to enable the engine to make the best time possible. It is not safe to run an imperfectly balanced engine beyond a certain speed.

To have any machine, or part of a machine revolving or oscillating about an axis, perfectly balanced, the principles of mechanics require an equilibrium of both the *centrifugal forces* and the *centrifugal couples.** This, then, furnishes us the basis for determining the necessary formulæ.

The weight of the reciprocating parts (piston, crosshead, etc.), should not be counterbalanced on the main drivers alone, but this weight should be distributed equally among all the drivers. The reason of this is readily understood, as the full force of this weight acts upon the wheels only when the crank is on its "centers." When the crank is vertically over, or under the axle, it has little or no influence in disturbing the equilibrium of the wheels as far as centrifugal force is concerned. The result of this will be that when the wheels are perfectly balanced on the "centers," as they should be, they will be overbalanced when the crank is at right angles to the center line of the cylinder. As this cannot be avoided, the balancing of the reciprocating parts should be distributed equally among all the drivers, thus distributing the over-balance among all the wheels. The most important point is to have the wheels well balanced *on the centers*, as it is the *horizontal thrust* which has a tendency to cause the engine to sway sideways, pressing the wheels against the rail, and thus making them liable to climb the rail which would result in the engine leaving the track. As far as the disturbing force on the "centers" is concerned, it will practically remain the same, if we suppose the weight of the reciprocating parts to be equally divided among the drivers and concentrated at the crank pins. This will facilitate the application of mathematics to the case.

Let us adopt the following nomenclature for the sequel:

W = weight of reciprocating parts—piston, piston rod, crosshead and main connecting rod.

P = that part of the parallel rod's (or rods') weight which is supported by the crank pin of the wheel under consideration.

w = combined weight of the counterpoise and crank of one wheel.

r = radius vector of w—*i. e.* the distance from center of axle to center of gravity of counterpoise and crank.

c = length of crank.

a = distance between middle of main rod connections, measured parallel to the axle, as shown in Fig. 2 = distance between centers of cylinders.

b = distance between center of gravity of counterpoise = distance between middle of "wheel centers"—Fig. 2.

* Rankine's Machinery and Millwork, pages 365-8.

d=distance between middle of parallel rod connections.

φ=angular velocity of wheels.

g=force of gravity.

θ=angle, which the line drawn from the center of the wheel through the center of gravity of the counterpoise and cranks makes with the crank line. See Fig. 1.

n=number of driving wheels on one side of locomotive.

In Fig. 1 is represented the general arrangement of the principal parts of a pair of wheels. Suppose planes A and B to pass through the axis $\overline{OQ}$ and the cranks $\overline{OA}$ and $\overline{QB}$ respectively—being at right angles to each other.

Now, the forces acting upon a wheel,

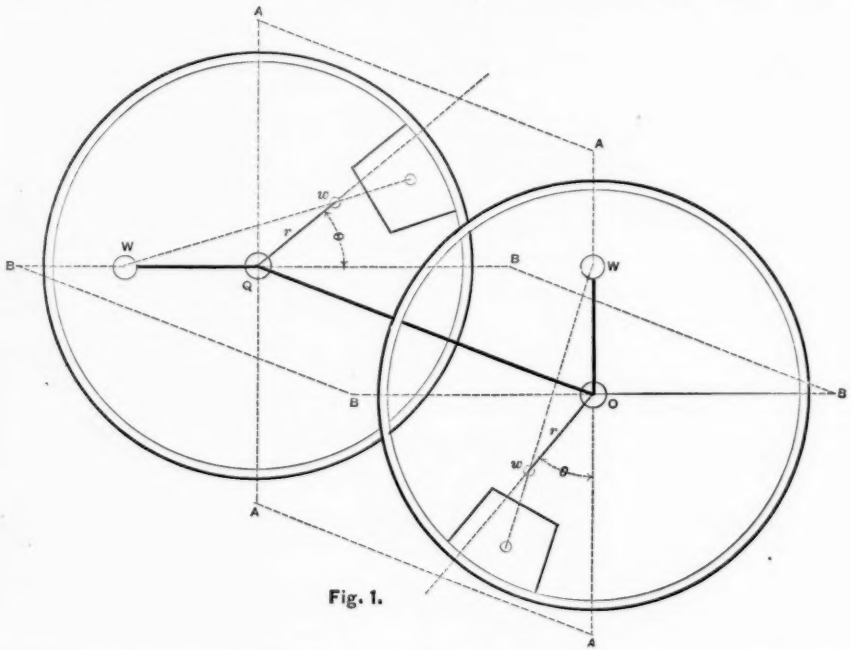


Fig. 1.

in a radial direction, at the instant the crank passes its center, may be divided into three distinct parts, viz.: the centrifugal force of the crank and counterpoise, acting at their joint center of gravity; the centrifugal force of part of the parallel rod acting on the crank pin,

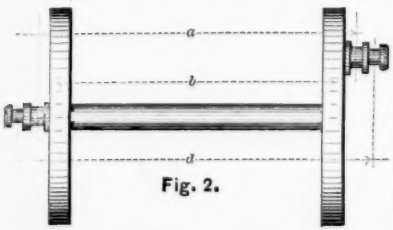


Fig. 2.

and the force due to the momentum of the reciprocating parts which may be considered equivalent, in effect, to the centrifugal force of an equal weight concentrated at the crank pin.

Resolving these forces into the planes A and B, Fig. 1—distributing the weight W as previously stated—we have

$$\frac{P\varphi^2 c}{g}, \frac{W\varphi^2 c}{ng} \text{ and } \frac{w\varphi^2 r\sin.\theta}{g}$$

as the forces acting in the plane B, towards the left. The opposing force in this plane is

$$\frac{w\varphi^2 r\cos.\theta}{g}$$

Equal forces act in the plane A. Now,

in order to have these forces balance each other we must have, first

$$\frac{W\varphi^2 c}{ng}+\frac{P\varphi^2 c}{g}+\frac{w\varphi^2 r\sin.\theta}{g}=\frac{w\varphi^2 r\cos.\theta}{g}$$

or,

$$\cos.\theta-\sin.\theta=\frac{c}{wr}\left(\frac{W}{n}+P\right) \quad . \quad (1)$$

Second, taking the origin at the middle of axle for centrifugal moments, we must have

$$\frac{W\varphi^2 ca}{2ng}+\frac{P\varphi^2 cd}{2g}=\frac{w\varphi^2 rb\sin.\theta}{2g}$$

$$+\frac{w\varphi^2 rb\cos.\theta}{2g},$$

or

$$\cos.\theta+\sin.\theta=\frac{C}{wrb}\left(\frac{Wa}{n}+Pd\right) . \quad (2.)$$

Combining equations (1) and (2) we have

$$\tan.\theta=\frac{\frac{W}{n}(a-b)+P(d-b)}{\frac{W}{n}(a+b)+P(d+b)} \quad . \quad (3)$$

From this equation can be determined the *angular position* of the counterpoise. Also by a proper combination of equations (1) and (2) we obtain

$$w=\frac{c}{r}\sqrt{\frac{\left(a\frac{W}{n}+Pd\right)^2+b^2\left(\frac{W}{n}+P\right)^2}{2b^2}} \quad . \quad (4)$$

which determines the *weight* of the counterpoise and crank. These equations are applicable to all cases in locomotive practice.

It is a general practice to place the counterpoise directly opposite the crank. If this is correct then the first member of equation (3) should be zero. This is only possible when $a=b=d$ or when

$$a=\frac{nP}{W}(b-d)+b. \quad . \quad . \quad . \quad (5)$$

But a, b and d can never be made equal to each other in practice and to have the relation of these quantities as expressed in equation (5) would require the engine to be "*inside connected*" as an examination of this equation will show. If these quantities would ever have this relation, it would only be by chance and hence it would possibly be correct, once in a thousand cases, to place the counterpoises *opposite the cranks, on inside connected engines*, but on *outside connected engines—never.*

Having now found the key for balancing locomotive engines, let us next consider

THE PRACTICAL OPERATIONS

of locating the counterpoises and adjusting them to the proper weight. This naturally divides itself into two parts:

I. To separately locate the counterpoises on the wheels, and to adjust their weights according to the formulæ already deduced.

II. To make a final adjustment by means of dynamical tests.

PART I.

Under this division of the subject we have two cases; first, solid or fixed counterpoises cast solidly into the wheels, and second, removable counterpoises.

Case 1.—The adjustment of solid counterpoises must be made before the

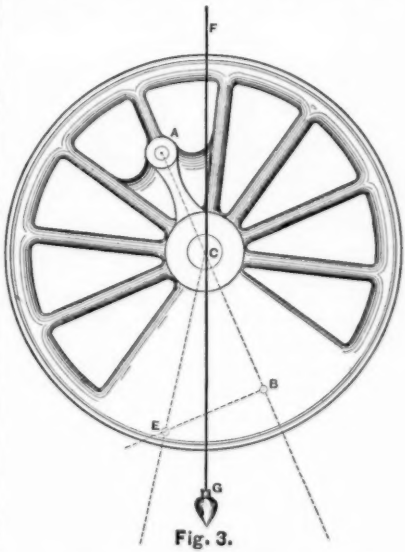


Fig. 3.

wheels are pressed upon the axle, otherwise the angular position of the counterpoise would be indeterminate.

To commence, then, locate the point B, Fig. 3, upon the crank line $\overline{ACB}$, at some convenient place. Next lay off, from B and perpendicular to $\overline{CB}$, a distance

$$\overline{BE}=\overline{CB}\ tan.\theta. \quad . \quad . \quad . \quad (6)$$

The value of $tan.\theta$ is obtained from equation (3). Having thus located the point E, fix its position with a prick punch. This gives the proper angular position of the counterpoise, viz.: $BCE=\theta$. The *sign* of $tan.\theta$, as found from equation (3) will determine whether the point E should be laid off on the left or right side of $\overline{CB}$. That is, when $tan.\theta$ is positive then the cranks and counterpoises should have the relation shown in Fig. 1, but if $tan.\theta$ is negative then E should be laid off on the opposite side of the crank line $\overline{CB}$. This, however, will only be the case with inside connected engines.

By a careful inspection of equation (3) and Fig. 1 we deduce the following rule:

Place the counterpoise (or the line $\overline{EC}$, Fig. 3), on that side of the crank line $\overline{CB}$ on which the crank of the opposite

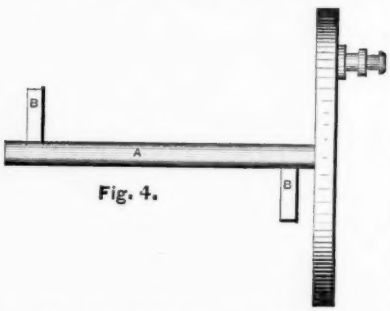


Fig. 4.

wheel is situated, for *outside connected engines*, and on the opposite side for *inside connected engines*.

For example, to locate the counterpoise on the wheel O, Fig. 1, we see that the crank $\overline{QW}$, on the opposite wheel, projects to the *left* of the crank line OA (or the plane A), hence the counterpoise (or the point E, Fig. 3), should be placed on the *left* side of the crank line OA if it is for an *outside connected engine*, but if it is for an *inside connected engine* the counterpoise should be placed on the *right hand side* of the crank line OA.

Having located the point E, Fig. 3, as explained, fix the wheel temporarily upon a shaft, A, Fig. 4, and place it upon leveled straight edges, B, as shown. Now let the wheel come to rest in its natural position, as in Fig. 3. Hold a plumb line $\overline{FG}$ over the center, C, of the wheel. If the plumb line does not pass directly over the point E, then the counterpoise is either too light or too heavy. If it should take the position shown in Fig. 3 and the counterpoise is too heavy, then some metal must be *removed* on the *right* of the plumb line; but if it is too light then some metal must be *added* on the *left* of the plumb line in order to bring it over the point E. To ascertain whether the counterpoise is too light or too heavy, turn the wheel to the position shown in Fig. 5, bringing the line $\overline{CE}$ to a horizontal or level position. Hold the wheel in this position by means of a spring balance or scales applied at the point E. Let w_1 represent the weight which should be indicated by the scales when the counterpoise is of the proper weight.

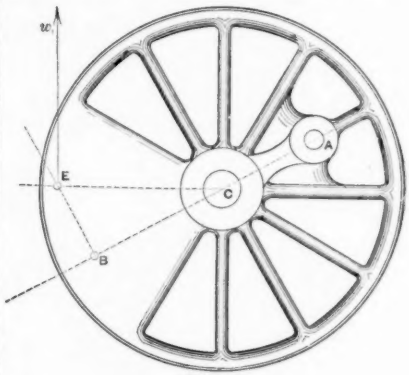


Fig. 5.

The value of w_1 may be obtained from the equation

$$w_1=\frac{c}{\overline{CE}^*}\sqrt{\frac{\left(\frac{aW}{n}+Pd\right)^2+b^2\left(\frac{W}{n}+P\right)^2}{2b^2}} \quad (7)$$

If the scales indicate more than the weight w_1, as computed from this equation, then the counterpoise is too heavy, and *vice versa*. When the counterpoise is so adjusted that the weight indicated on the scales is equal to w_1, as computed from equation (7), and the plumb line, Fig. 3, passes through the point E, then it is adjusted as it should be. Care must be taken not to locate the point E on the wrong side of the crank line. To avoid this mistake it must be first

* The distance $\overline{CE}$ in Figs. 5 and 3.

decided upon, and clearly borne in mind, what situation the wheels will have when pressed upon the axle. Of course this must be done before the rule of page 12 can be applied. If the crank pin is not on the wheel when the adjustment is made, then its weight should be added to $\frac{W}{n}$ in the equation before the calculations are made.

Case 2.—In case of removable counterpoises we may temporarily bolt them in their proper places, and then proceed as in case 1, if the adjustment is made before the wheels are pressed upon the axle, but if the wheels have been pressed upon the axle previous to the adjustment of the counterpoises, we must proceed as follows:—

After the counterpoises have been fitted to their places between the spokes of the wheel, remove them and bolt each pair together as seen in Fig. 6—No 1. Now locate the center of gravity of each pair on the outside. This may be conveniently done by suspending them with a grip-hook, as shown, and suspending a plumb line from the point of the hook. Mark the position of the plumb line, as $\overline{AB}$, Fig. 6, No. 2; then suspend the

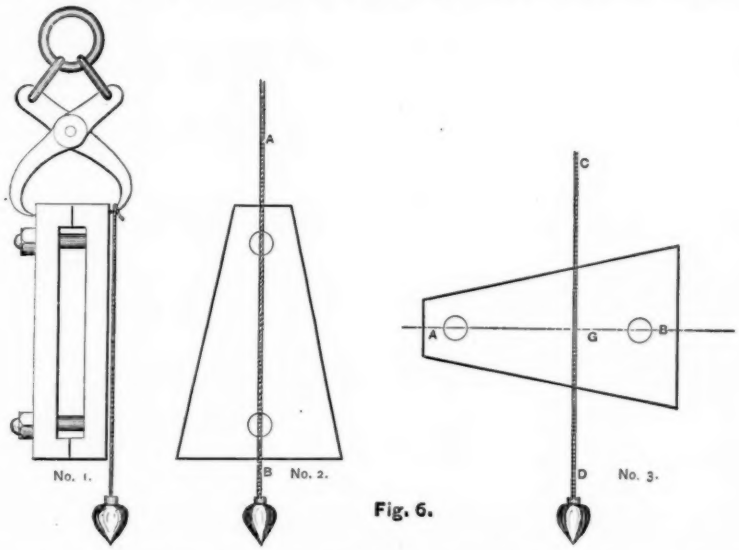


Fig. 6.

counterpoise in another position, and mark the plumb line as at $\overline{CD}$—No. 3. The intersection, G, of these two lines will be the center of gravity sought. Now weigh each pair separately, and mark the weight upon it. Next place the wheels upon leveled straight edges, before the counterpoises have been put in place, and turn them so that the crank line of one wheel shall be in a vertical position, then the other will be horizontal, as in Fig. 1. Hold the wheels in this position with the scales applied to the crank pin of the horizontal crank. The weight indicated on the scales in this position will be a weight which, when applied at the crank, will have the same centrifugal force as the crank itself. It may, therefore, be substituted for the latter. Let us denote this weight by m. Now fasten the counterpoises in their respective places between the spokes, as in Fig. 7. Let G_1 be the center of gravity of the first pair, G_2 that of the second, G_3 of the third, etc. Also let w_1, w_2, w_3, etc., be their respective weights. Mark the line $\overline{CE}$ upon the wheel, the angle $\overline{BCE}$ being determined as in case 1. Now find the center of gravity of the counterpoise and crank combined as follows:—

Join G_1 and G_2 and lay off from G_1 the distance

$$\overline{G_1F} = \frac{w_2 \times \overline{G_1G_2}}{w_1 + w_2} \quad . \quad . \quad . \quad (8)$$

Next lay off on the line FG_3

$$\overline{FH} = \frac{w_3 \times \overline{FG^4}}{w_1 + w_2 + w_3} \quad . \quad . \quad . \quad (9)$$

and finally,

$$\overline{HG} = \frac{m \times \overline{HA}}{w_1 + w_2 + w_3 + m} \quad . \quad (10)$$

The point, G, thus found is the center of gravity sought—of the counterpoise and crank. The weights, w_1, w_2 and w_3, should be so adjusted that the point G will fall upon the line $\overline{CE}$ and the *sum* of the weights w_1, w_2, w_3 and m equal the weight w as computed from equation (4), in which we must substitute, for r, the length CG. Fig. 7.

This method is not as accurate as that of Case 1, and it is at the same time a great deal more tedious, but it is the only way we can make the adjustment after the wheels are on the axle.

In making patterns for wheels the

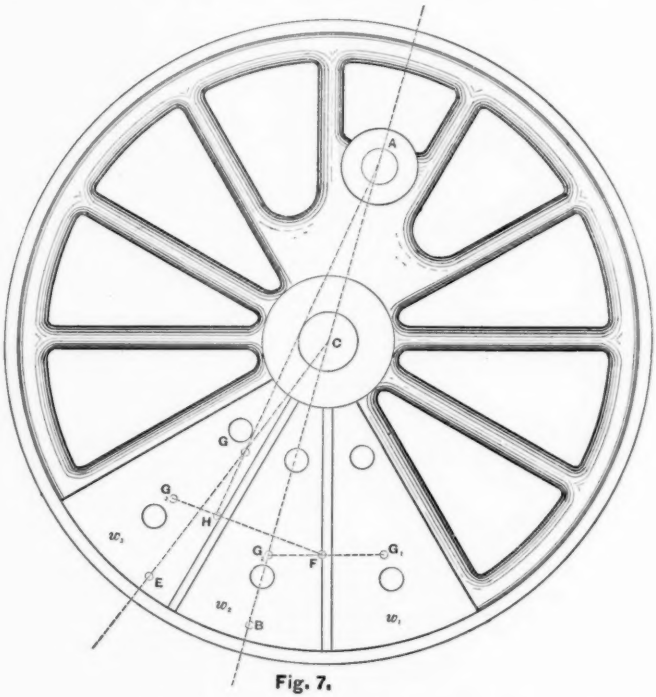


Fig. 7.

weight and position of the counterpoises may be approximately determined in the same manner as herein set forth, by multiplying the results obtained from the equations, before using, by 0.092 if pine wood is used.

Of course it will be unnecessary to remark that the wheels can be given a symmetrical appearance, although the center of gravity of the counterpoises is set to one side of the crank line. This can be done by simply making the counterpoises heavier on one side of the crank line than the other.

Part II

FINAL ADJUSTMENT.

Although the adjustment of Part I. will give a better result than is obtained by the ordinary practice it will not be perfect, owing to the impossibility of taking into account various slight causes

which have an influence in modifying the action of the machinery. The adjustment may, however, be much improved by making what may be termed *dynamical tests*, with which we may proceed as follows:—

After the counterpoises are adjusted according to Part I. and the engine is ready to "fire up," it should be suspended by four points of its frame from some rigid frame-work, sufficiently high above the track so it may swing freely. Fix a pencil on a spring and fasten the latter to the engine at some convenient point. Next take a board, upon which has been mounted a piece of paper, and fasten it to some stationary object immediately beneath the pencil point, in a horizontal position. Fasten also a piece of chalk, by means of a spring, at some convenient point near one of the driving wheels. Connect the pencil and chalk in such a manner that the former

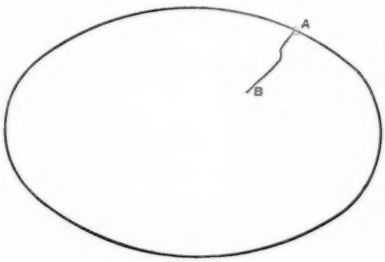


Fig. 8.

may be drawn to one side at the same instant that the latter is pulled against the wheel. This may be done by means of a string. Everything being thus prepared, turn on the steam and set the wheels in motion—the driving boxes having been previously blocked up. As the machinery is thus in motion, the engine will swing in an orbit, the form of which is traced by the pencil point when brought in contact with the paper. This orbit will generally be elliptical in form, as shown in Fig. 8. At any time while the machinery is in motion, suddenly pull the string connected with the pencil and chalk, so as to make, simultaneously, a mark on the wheel and across the orbit as AB. The point A will, of course, indicate the position of the pencil point at the instant the string was pulled. The position of the wheels at the same instant is determined by simply turning them, until the chalk-mark made stands opposite the piece of chalk. Having thus determined the relative positions of the pencil and wheels at the same instant, we can at once see whether the counterpoises are still too heavy, and more metal may then be removed, or *vice versa*.

If an engine could be perfectly balanced the orbit described by the pencil point would be reduced to a point; that is, the engine would remain perfectly at rest, however rapid the motion of the machinery might be. But this condition of things can never be realized in practice. We can only make *perfection* our objective point towards which we will work, and the man who can approximate nearest to it is the "biggest success." Prof. Rankine* seems to think that the approximation may be so near to perfection that the diameter of the pencil's orbit be reduced to one-sixteenth of an inch. This would, indeed, be a very satisfactory result, and beyond our expectation.

Chinese Steel.—A considerable steel-making industry exists in the present day in China, on the Upper Yangtze, whence the steel is sent to Tient-sin for shipment and distribution. It fetches much higher prices than the Swedish steel imported into the country. The Chinese metallurgists recognize three kinds of steel—namely, that which is produced by adding unwrought to wrought iron while the mass is subject to the action of fire; pure iron many times subjected to fire; and native steel, which is produced in the south-west. The different names for steel are twan kang, or ball steel, from its rounded form; kwan kang, or sprinkled steel; wei tee, or false steel. The Chinese, apparently have known how to manufacture steel from the very earliest ages, and in the time of the Hau dynasty iron-masters were appointed in several districts of the old Leangchou to superintend the ironworks.—*Iron*.

* Rankine's Steam Engine, page 531.

DETERMINATION OF THE THICKNESS AND FORM OF THE ARCHES OF STONE BRIDGES.

By G. TOLKMITT.

(Zeitschrift des Architekten-und Innegieur-Vereins zu Hannover.)
Translated for Institution of Civil Engineers.

The thickness necessary for the arch of a stone bridge depends on the strength of the material of which the arch is built, the load it has to bear, and the form and dimensions adopted for the archring. All empirical formulæ, used for determining the depth at the crown, which do not contain these three factors can only be regarded as giving adequate approximation when applied within narrow limits. If the span of the arch be small, and the backing above it of little depth, it is necessary besides to take account of a partial distribution of the live load. It may generally be said, that the thickness required will be least when the form chosen for the arch is such as to make the line of pressure which corresponds to full loading coincide with the mean line of the archring. In a former paper, published in the "Zeitschrift für Bauwesen" of 1876, the author has gone fully into this question; and the results then obtained have been collected and reproduced in a small table. The chief object of the present paper is, however, to establish a formula which, while taking due account of the three factors, gives also an easy means of determining the thickness at the crown of such arches. The formula is not strictly accurate, but numerous applications which have been made of it have proved that the approximation, even in extreme cases, is very great.

The author examines two distinct cases: 1st. The evenly distributed live load, covering the entire length of the span. 2nd. The live load extending only from one of the abutments to the center of the arch. The latter case leads generally to greater thicknesses than the first, but still it is always advisable to try both hypotheses. If c be the thickness at crown, it can be expressed in the first case by

$$c=\frac{.15\times\frac{w^2}{f}}{q-.15\,\frac{w}{f}}\left(e+p+\frac{f}{10}\right).$$

e, depth at crown of the surface, representing the permanent load on the arch. It is comprised between the extrados and a horizontal line above it.

p, depth of horizontal strip, representing the live load.

w and f, span and rise of the intrados of the arch.

q, greatest pressure admissible on the stone.

The formula is independent of the choice of unit, which may be at pleasure the foot, the yard or the meter. Although q is equivalent to a weight, it is not to be expressed by a number of units of weight, but by the volume of stone of a like weight. Thus, supposing the stone to be able to bear safely 80 tons per square meter, and to weigh 2 tons per cubic meter, the numerical value of q to be introduced into the formula will be $\frac{80}{2}=40$ cubic meters.

In the second case, that of partial distribution of the live load, the expression of c depends on the nature of the assumption which is made with regard to the springings. When they are assumed as rigidly fixed, the value of c will be smaller than when they are supposed to admit of slight angular displacements. It is, therefore, judicious to adopt the second hypothesis, although probably farther from the truth than the first. The equation becomes in this case,

$$c=.625\times p\,\frac{f}{c+e+\frac{p}{2}+\frac{f}{10}}\times\frac{2w-f}{2w}.$$

The symbols have the same signification as before. The thickness at the crown being computed, there remains to draw the arch in its right form. This is done by means of the table already cited. The operation is extremely simple, reducing itself to the multiplication by numerical co-efficients of a series of figures contained in the table.

COLOR BLINDNESS IN RAILWAY EMPLOYEES.

Abstract of Report of Massachusetts Railroad Commissioners.

THE Board gave early attention to this subject, witnessing an interesting examination of railroad employés, conducted by Dr. Jeffries, and listening to his explanation of his theories and methods. They also united with him in sending circulars of inquiry to various experts and officials in Europe. They have examined a large number of persons, chiefly employed by several railroad companies, using for tests, colored worsteds, as recommended by Professor Holmgren, and as practised by Dr. Jeffries. This examination was supplemented by experiments with colored flags and lanterns whenever any visual defect was found to exist. They have also sought, by reading and conversation and correspondence with persons practically interested in the matter, to obtain information on the subject.

Any one who engages in the study, will find himself anticipated in every direction by Dr. Jeffries, who has pursued it with unwearied industry, and to whom the community is deeply indebted for his labors. If he has exaggerated the importance of his subject, or the frequency and extent of the defect which he discusses, it is the natural and honest exaggeration of an enthusiast devoted to a specialty in science.

It has long been known that there is such a defect in vision as color-blindness. In rare cases it is total; more frequently it is partial. Various divisions have been made by different writers in describing this defect. Professor Holmgren's division is as follows: I. Total color-blindness. II. Partial, which he subdivides into: 1. Complete color-blindness, including red-blindness, green-blindness and violet-blindness. 2. Incomplete color-blindness, where the sense as to one or more colors is feeble. His divisions are cited, for he is often quoted; and it is necessary to remember that with him one subdivision of "partial color-blindness," is "complete color-blindness," as this phrase is likely to mislead. The different species of this defect, practically important in connection with railroads, are total color-blindness, which is very rare, red-blindness and green-blindness. The red-blind confound red with green, with gray or brown, and sometimes with black. The green-blind confound green with red, or with gray and brown. The inability to distinguish red and green, is not only the most marked species of color-blindness, but it is in practice the most important, as these colors have been very generally chosen to signify danger and safety on railroads, and universally to mark port and starboard on the sea.

Generally the defect exists from birth, being often hereditary. The learned Dr. Pliny Earle, who wrote a valuable article on this subject in 1845, knew seventeen persons of different generations in his own family, who were utterly unable to discriminate between red and green. Sometimes it is caused by sickness or injury; and frequently it results from the excessive use of liquor or tobacco. Often the defect is unsuspected for years. This happened in the case of the famous John Dalton, whose name has long been connected with this peculiarity of vision. It is the general belief in Rome that Garibaldi selected the red flannel, which increased the exposure of his soldiers to the marksmen of the enemy, in full belief that it was dark green, laying his hand on a scarlet piece of cloth instead of a green one, when choosing the material for their uniform. Even if the story is unfounded, it shows that the existence of total color-blindness is recognized and notorious, not only among men of science, but among all classes of people.

It need hardly be said that such a defect is a source of danger, while railroad trains are run by colored signals. It is true that the commissioners have not been able to find that any railroad accident has ever been clearly traced to this cause. Professor Holmgren indeed says that, in a certain trial, "testimony was adduced which had led me to suppose that color-blindness was one of the principal causes of the disaster." He

and other writers speak of it as a cause of accident in other countries, or in distant places. *The Scientific American*, in the number for July 9, 1853, refers to color-blindness as a possible cause of the terrible Norwalk accident of May 6, 1853, by which forty-six persons were killed. But no investigation on this point seems to have been made, and upon examination the facts do not appear to warrant any such conclusion. The same excellent journal had called attention to the subject in its number of May 28. The director of the Swedish State Railroads, C. O. Troilius, who has given especial attention to the subject, writes: "No accident traceable to color-blindness, as far as we know, has occurred on our lines." One of the railroad journals of this country, *The Railway Age*, in its investigation of the subject, has received answers from thirty-seven superintendents, and other managers of railroads, no one of whom had ever heard of an accident resulting from color-blindness. This, of course, does not prove that no such accident ever did occur.

The possibility of accident arising from this cause has been shown. And as to one employé, recently for the first time discovered to be color-blind, it appears that on former occasions he has led several engineers into errors, for which they have been reprimanded. No doubt now exists that he did this by displaying the wrong signals. Each of these errors might have resulted in disaster. The defect was unknown to the man himself, but was manifest at once on his being examined by the intelligent officer detailed by the superintendent of the road for that duty, and whose skill and thoroughness is elsewhere spoken of. It is hardly necessary to add that the person examined is now in a position where perception of color is not needed.

In *The Chicago Railway Review* for March 30, 1878, is a communication expressing a belief that many accidents have been caused by this defect. And the writer speaks of one case known to him, where an engineer who had had many narrow escapes, finally proved to be color-blind, and acknowledged the fact. But each of these narrow escapes might have been a disaster, and the case shows the need of examination as clearly as if fatal accidents had occurred.

The investigations of the board have convinced them that while danger is possible, and while all needed precautions to avoid such danger are an absolute duty, yet its extent has been greatly exaggerated. When a large per cent. of color-blindness is reported among a body of employés, great allowance should be made for the agitation and nervous excitement under which they labor when they are examined. They are called in from the open air to a room perhaps imperfectly lighted, and in the presence of strangers are subjected to an investigation which is mysterious to them, and of which they have heard that their daily bread and that of their children may depend upon its result. While they are thus "on trial for life" they often make mistakes which wholly misrepresent their capacity for distinguishing color, and which give a false idea of their general intelligence.

Of course, if it is unsafe to employ a man afflicted by color-blindness, he must be discarded. It would be false sympathy and misguided pity that would retain him. But it would be at once foolish and cruel to remove three or four per cent. of our railroad employés from their places, if they are in fact fully qualified to perform their duties.

The use of the phrase "color-blindness" tends to mislead. It is applied to all persons whose sense of color is in any way deficient. The word "blind" is not so used, as applied to men whose vision is defective. We do not call near-sighted persons "blind." As all would agree that blind men are not fitted to run an engine, so without an explanation of the term, it would seem plain that color-blind men, especially when afflicted with "complete color-blindness," should not be employed, when it is necessary to distinguish the color of signals. But the word as used by specialists, includes persons who, in the ordinary walks of life, and in their special employment on railroad trains, do, habitually and accurately and without failure, distinguish all colors, but who, upon examination, sometimes erroneously select as having a green or red tinge a "color of confusion." And knowing this, we may well hesitate before we reject from railroad service, all who are pronounced "color-

blind." Professor Holmgren himself says: "He whom we call color-blind is not, correctly speaking, at all blind to colors.

The public are also liable to be misled by witnessing experiments with persons totally color-blind. There are a few such persons; and their efforts to select and match colors furnish a striking and amusing exhibition. When we see green matched with scarlet, or a bright red skein of worsted confidently described as black, we are naturally impressed with the visual imperfection of the person on trial. And when we are told that four or five per cent. of the male population have this defect, and that up to this time, there has been little or no examination of railroad employés, the effect is to shock and alarm us. But the alarm is diminished when we learn that out of a hundred persons whom science declares to be "color-blind," not one may be incapable of rapidly and correctly distinguishing one color from another.

It is also to be remarked that color-blindness does not imply indistinct vision in other respects, but is often attended with a quicker perception of faintly illuminated objects. And it is another ascertained fact, the reverse of what some might expect, that color-blindness partially disappears when colored objects are illuminated by artificial light. And, therefore, "contrary to common belief, our present railway signals are safer, so far as liability to mistake by the color-blind is concerned, by night than by day." But it is added, "though safer they are not safe."

This is one reason why persons who have been pronounced to be color-blind, prove, on examination, to have full perception of the colors of lanterns when placed at great distances and under trying circumstances. Such has been the experience of the commissioners who have thus experimented on railroad employés who are theoretically color-blind, and who promptly distinguished white, red, blue and green lights at a great distance, while engines were going out and coming in, with all the attendant annoyance of smoke and steam. The same men, also, distinguished by daylight, red, green, and white flags, at a like distance without failure, while a person totally color-blind who happened to be present on one occasion, pronounced a scarlet flag to be black, when it was held directly before his face.

One explanation of this combination of theoretic color-blindness with real and unerring sharpness of color-vision, may be found on examining the plates and directions used by scientific men to illustrate this subject. In Professor Holmgren's plate, as published by Dr. Jeffries, will be found the green test I followed by five "colors of confusion." And on p. xix, it is said: "If the person examined takes any of the confusion colors (1 to 5) to put with the green, he proves himself color-blind; *or even if he seems to want to put them together.*" But No. 1 of the confusion colors seems to many persons of perfect vision—perhaps to a majority of them—to contain, mingled with gray, a slight tinge of green. And this incident occurred at one examination. A railroad employé had shown great readiness in picking out different shades of green, and finally selected a skein of worsted corresponding to No. 1. Upon this, an expert in color who was present, remarked that the man was also an expert, and declared that he was the keenest in vision of all that had been examined. Yet, this man had proved himself to be technically color-blind, and so, unfit to earn his living by doing his daily work on an engine.

It is said of some of these "color-blind" persons who run engines with success, that they "guess" at colors by the varying intensity of light,—that they do not see colors properly or as persons of normal sight see colors. But if they always judge rightly no one cares whether they see as we do, or not. Dr. Holmgren says: "Just how a color-blind sees a color it is not possible to decide; for it is a subjective sensation." The only practical question is, whether these persons in fact, can rapidly and unerringly distinguish one color from another.

It is suggested that, when men judge of color by the relative intensity of light, they are liable to be misled, when fog, or sleet or smoke obscure a signal lantern, and so diminish the intensity of its light. And this would seem to be a reasonable suggestion. Thus Dr. Spalding, who

examined the men employed on the Maine Central, is reported as saying, that to a color-blind man a fog may turn a red light into green. But he does not state as a fact that this ever occurred. And in regard to a color-blind engineman, whom he examined, the testimony was that he never failed, under any circumstances, to distinguish red signals from green. Nor is any case recorded, so far as is known to this board, of a "color-blind" man who could distinguish red lights from green in clear weather, and who has mistaken red for green in foggy weather.

The commissioners tried a number of experiments upon persons who failed in the ordinary tests of color perception, by placing at distances of five hundred or seven hundred feet, red lanterns muffled, doubly muffled and obscured by a smoke-colored fabric; with the idea that this artificial obscuration of light would have the like effect with that of snow, or sleet or mist. But in no case did any man, who could distinguish the lights at that distance, confound the obscured light with green, or hesitate to decide rightly on the color. A smoked white glass was used with the same effect.

But the one man who was found unable to distinguish colored lights at a distance, did repeatedly mistake a muffled red light for green, and also a smoked white lantern. It might be thought that he always guessed at random and only happened to repeat these errors. But he repeated them on more than one occasion, and it seemed that there must be a special cause for these special blunders. His case seems to confirm the theory that persons really color-blind do judge of colors by their relative intensity, giving them names associated in their minds with that degree of intensity. Such a man would, of course, be utterly unfit to take any part in running a train; nor would he be so employed by any manager cognizant of his defect.

The effect on the vision of color-blind persons of increasing or reducing the intensity of light has been differently stated. A case has been recorded of a red-blind man who could, for his eyes, change the white light to green by screwing down the wick, and who made it red by screwing it down yet further. But to most persons red seems more brilliant and intense than green; and so it is stated by Professor Holmgren, speaking of all the color-blind. In another portion of his work, however, he says that to the green-blind green is weaker than red, while to the red-blind red is weaker than green. Probably all the peculiarities of this defect are not yet known; and the commissioners regret that this season has not yet afforded them opportunities for trying practical experiments on color-blind persons in snowy or even in very foggy weather.

It has been suggested, that in railroad matters all difficulty on the score of color-blindness might be removed: (1.) By selecting other colors as signals, and by discarding red and green; or (2.) By using signals differing in form instead of color, in order to indicate danger and safety. But it has been proved to be impracticable to dispense with red and green. Blue is objectionable because blue glass intercepts so many rays of light that it becomes very feeble, and can only be seen at a short distance; yellow is too near akin to white, as all white lights have some yellow; black is of no service at night; and it need not be said that night signals are the most important.

Nor can form alone be well used to designate safety and danger, because a difference in color is seen sooner than a difference in form, and more persons would fail to distinguish form at a distance than would fail to distinguish color. Of course a difference in the form of signals, as well as of their position, may be used as an auxiliary to the difference in color, and both are so used with good effect; but the whole body of railroad men and managers would protest against discarding green and red as signals.

Efforts not wholly unsuccessful have been made to remedy the defective vision of the color-blind by the use of colored glasses, or of glasses enclosing a colored liquid; but up to this time no device has been found that would be satisfactory in case of real color-blindness. A similar statement may be made as to the special education of color-blind persons. Something may be done to diminish the defect, but no amount of education in color would fit a person

really color-blind to drive an engine; and the board agrees with Professor Holmgren in his conclusion: "As long as the existing system of signals used on railroads is considered in almost all respects the best known, it is indispensable that no one incapable of rapidly and accurately distinguishing red, green and yellow should be allowed to fill any position on railways involving any connection with colored signals." The question which has been found difficult to decide is: Who are so incapable?

A brief statement of the laws and practice in various countries as to examinations may be of some value.

In England no law exists which requires examination; but the principal railroads, and perhaps all, examine their employés for color-blindness, and accept no engine-drivers or signal-men without previous examination. The method, however, is simple; few men are rejected, and the tests applied are not considered as sufficient by those especially interested in the subject.

In the Cunard line of steamers an examination is held previous to every departure of a steamer from Liverpool. This is done simply by holding a board, marked with various colors, at a short distance from the person examined, and asking him to name them. The examination is made of common sailors as well as of officers. In the Leyland line all candidates for position as master, first and second mate, are examined as to their knowledge of colors by the same person who examines in navigation. Pieces of glass are used, colored green, dark green, red, blue, sky-blue, yellow (dark and light), and white.

In Sweden, no law has been passed compelling an examination, but no one is allowed to enter the service of the state railroads until he has been found faultless in the faculty of discerning colors. Since 1876 all have been examined, nearly three per cent. being found more or less defective.

In Holland, the regulations require examination for railroad employés, as well as for the naval and mercantile marine.

In Germany, examinations have been officially recommended, and they have been made, but not in a manner satisfactory to those who have made the matter a specialty. The tables, as republished by Dr. Jeffries, show that only 319 color-blind persons were found among 41,444 examined, being considerably less than one per cent.

In Italy, no regulation is published, but inspectors examine applicants for employment as to their power of distinguishing colors by natural light, and by lamp-light. Apparently the examination is practical, and not scientific or technical.

In France, no law seems to have been passed, but examinations are made on many of the railroads.

At the meeting of the International Medical Society, held this year at Amsterdam, a code was reported by Professor Donders for regulating this matter, and was recommended by the society.

In this country, examinations have been made by many railroad companies since attention has been called to this subject. Dr. Keyser, of the Wills Eye Hospital, is reported as having examined in eight months all the employés on all the roads terminating in Philadelphia, excepting the Pennsylvania Railroad. His report was, that three and a half per cent. "have defects of such a character as to make them really incapable and unsafe to fill the positions they occupy." He found, also, that an engine-man who was green-blind never mistakes as to the color of signals. In Maine, an examination heretofore referred to was made on the Maine Central Railroad, with results not unlike those of Dr. Keyser.

In the United States army, all accepted recruits are now examined for color-blindness, and defects are noted, but are not cause for rejection, except for the Signal Corps. Examination is made by the use of test-wools, according to Professor Holmgren's method. The object of the examination is stated as being twofold: to avoid assigning to the color blind duties for which they are unfitted; and the accumulation of facts to show whether further restrictions are needed on their enlistment. For the report on this subject the board is indebted to the kindness of Surgeon-General Barnes.

The board early became convinced that the possible dangers of color-blindness were such that examination ought

to be made at once of all the persons employed on our railroads, whose duties are in any way connected with signals. A circular was accordingly sent to the president of every railroad company operating a road, asking that all such employés might be examined, and the results reported to the board. One object of this was to prevent accidents arising from color-blindness. Whatever view might finally be taken as to the extent of the danger, the mere existence of it seemed to call for action without delay. Valuable information, also, was expected from the results of these investigations. And this hope was not disappointed. Reports received from the various roads show that intelligent and careful investigations were made on most of them. The commissioners have availed themselves of the information thus obtained, and have, where it was possible, followed up the experiments by further tests of persons found defective.

A striking result of these investigations was the effect upon the railroad officials who conducted or witnessed them. There had been much scepticism among them as to the existence of color-blindness. The experiments opened their eyes to a source of danger hitherto unknown, and insured attention to this matter in the future.

The board has been criticised for advising these examinations, made by "laymen," as distinguished from medical or ophthalmological experts. But the board was not authorized to direct the employment of such experts in advance of any law upon the subject. And, what is more to the purpose, the commissioners were soon convinced that any man of ordinary intelligence could conduct such examinations—at least the preliminary ones—so as to secure practical and valuable results. Printed directions are given in various works on this subject. They may be found on a page of directions issued by the medical department of the army. These are addressed to all medical officers. But, if these investigations were as critical as they are sometimes supposed to be, only experts in ophthalmic science could conduct them; and it might be doubted whether even such experts were skilled in the specialty of color-blindness, and, indeed, whether this state contained more than one competent person. The commissioners have not felt that they were shut up to register the views of any one expert. The legislature referred the matter to three "laymen" for investigation, assuming that they were competent to study it; and they are convinced that, so far as practically necessary, any man of average sense can test an employé so far as to learn whether it is safe to trust his sense of color in railroad service. Most persons, on examination, speedily prove their freedom from defect. The army directions provide that a green test-skein of worsted shall be laid aside, and the recruit shall be requested to place alongside of it all the shades of that color. And, "*if he promptly selects the shades of green only, then, after he has thus selected eight or ten skeins, the examination may be discontinued, for he is not color-blind.*" Now, almost all persons do this; and it is evident, that, so far as this test is concerned, it can be applied by any one who is not color-blind himself. There is no mystery in the use of this test,—no need of ophthalmic or medical knowledge. When the party fails to select the right color promptly, and appears on final examination to be defective, then he may well be allowed a further critical and medical examination, to ascertain whether he is really so defective in color-sense as to be unfit for employment. This is the course pursued on one of the best managed railroads in the United States. And their experience teaches what the limited researches of the board had already shown, that color-blind men can be detected without the possession of any special skill of learning.

The most rigorous examination, and the most complete report, of which the board has any knowledge, was made by a conductor who was detailed by one of the railroad companies for this purpose. This record was made instructive and interesting by his preserving, in each instance, a portion of the worsted which the employés selected as containing the colors offered as tests. The board would not have advised the selection of a conductor, lest he should be suspected of favoritism, but in this case a result was secured which was impartial, intelligent, and full of instruction.

The circular called attention, also, to defects of vision not relating to color. This subject was not referred to the board, but it is part of their ordinary duty to report on every important matter connected with the operation of railroads. And it is a striking fact that so little attention had been given to this subject. Most railroad companies seem to employ men for places where good sight is vital, without examination; and continue to employ men whose sight is likely to be failing from age, without testing their visual power. Probably their reasoning is that of an intelligent official, who writes: "We had one engineer who was near-sighted, and removed him at once; bnt he was the only man I have ever known who could not see an object six hundred feet from him that was willing to risk his life on a locomotive. There are many persons who are near-sighted, and a small percentage of the people are color-blind; but I do not believe they prefer railroading as a means of living."

But such reasoning is unsafe; and men whose vision has gradually become defective, without being conscious of it, are found employed on railroads; and it is probable that men who know their defect are willing to risk their lives, and the lives of others, rather than to lose their means of gaining a livelihood. Members of this board, while they cannot speak of any railroad accident as resulting from color-blindness, do know cases where defective vision has led to such accidents. In one case, at least, the defect was never suspected by the person himself, until it had caused a considerable destruction of property. It was then recognized; and a new employment, not requiring vigorous eye-sight, was given to the employé.

It has seemed to the board that examination as to strength of vision was even more important than examination as to color-blindness. And this view is confirmed by Prof. Holmgren's statement: "If the system of signals were based upon form, and all persons discharged from the service of railways, who, in consequence of an imperfection in vision, could not clearly and decidedly distinguish these signals at a distance, the proportion of such would be larger than that of the color-blind" (*Smithsonian Report for* 1877, p. 172). Of course, examination as to the two points can be made at the same time; and such was the recommendation of the circular.

This view has been further confirmed by the railroad officials who have tested their men, and who have found more defective in vigor of sight than in perception of color. In these examinations, men who failed to distinguish letters at a very short distance showed themselves far-sighted and clear-sighted in recognizing signals made at a distance of three thousand feet, and even of a mile. And these employés, technically defective, appeared to be practically well fitted for their duty. This, of course, was to have been expected from well-known facts as to vision, especially with those whose eyesight is affected by old age.

The final conclusions of the board are: 1. That the existence of color-blindness, total and partial, is a well-established fact, and that there are men who, by reason of such defect, are unfit for positions on railroads requiring ability to distinguish color-signals. 2. That the extent of dangerous color-blindness, i. e., such color-blindness as unfits persons for railroad employment, has been greatly exaggerated, and that a very small per cent. of persons are, for this reason, unfit for such employment. 3. That examinations may be properly made by persons not medical experts; and that such examinations will certainly be sufficient, if doubtful cases are referred to such experts. 4. The board recommends that every railroad company shall have an annual examination of every employé whose duties require or may require capacity to distinguish form or color-signals, and that no one shall be so employed who has not been thus examined. The examination should refer to color-blindness and to other defects in vision. It should include all who are in any way concerned in the movement of trains. 5. The board does not recommend legislation on the subject. The interest of each corporation is strong enough to insure careful examination. Humanity would prevent any company from knowingly employing a person whose defective sight might at any time cause a fatal accident. And self-interest will make railroad managers careful in avoiding

even false charges that accidents have resulted from such defects.

The failure to make examinations heretofore is owing to the want of information on the subject; and, in regard to color-blindness, to the general incredulity as to its existence. Information is now generally diffused, and incredulity has ceased, thanks to the efforts of scientific men. And there is no reason to fear that due attention will not be given to the recommendation that all applicants for employment on railroads, and all persons employed, shall be examined for defects.

PRODUCTION AND TRANSMISSION OF POWER BY ELECTRICITY.

By GEORGE W. BLODGETT.

From Papers of Boston Society of Civil Engineers.

The successful introduction of the electric light for practical use, the many inventions involving one or another of the applications of electricity, together with a popular interest in the many practical uses to which it can be put, make an examination into the methods, economy, and cost of its production and distribution, highly opportune. It is only within a few years that means have been devised to produce electricity in large quantities cheaply enough to come into use, even for lighting purposes. Now there are companies which engage to light mills, manufactories, and large areas, and guarantee the cost not to exceed one half that paid for gas, for the same premises, and furnish a better and purer light. Electricity is likely to be economically applied for many other purposes for which it is not now used.

It is not my purpose to discuss electric lighting, or the questions of great scientific and practical interest connected therewith; but since electric currents used are almost always generated by mechanical power, a description of some such machines, the mode of working, the degree of efficiency attained, and the relative merits of each type of machine which has been practically tested, may not be uninteresting. The sources from which electricity can be derived are almost innumerable; those best known being batteries of many kinds, frictional machines, thermopiles and electric machines. It is only the last which have been economically used for the production of large quantities of electricity. I ask you to take for granted that large quantities of electricity can be generated cheaper and more conveniently by mechanical means, than by chemical action or by friction. There are many kinds of machines, in all of which there is one important principle, known as the principle of induction. Machines can be divided into two classes: those that employ permanent magnets, and those in which the electricity which the machine generates is made to pass through long coils of wire which surround cores of soft iron, making the iron strongly magnetic, and forming what is known as an electro-magnet. Machines of the first class are called magneto-electric, and those of the second class dynamo-electric; their history is briefly as follows:

In 1819, Oersted, a Danish physicist, discovered that a current of electricity flowing in a wire near which was placed a magnetic needle caused a deflection of the needle.

In 1831, Faraday discovered that a magnet in motion near a coil of wire could generate a current in the wire.

These two discoveries, and those which followed, convinced the experimenters of that time of the general principles underlying them, which may be briefly stated in the following terms, and which is the law of the relations between electricity and magnetism:

I. Any variation in the electrical state of bodies can produce a magnetic disturbance, and any change in the magnetic condition of bodies produces corresponding electrical variations.

II. Magnetism may be induced in bodies capable of magnetic influences by magnets, and electric currents may be induced by the action of electric currents in other bodies.

The currents in magneto-electricity are called "induced," to distinguish them from those flowing from a battery, because they are usually not continuous, but are the result of a previously determined set of conditions. The first machine the writer has found any description of, caused a horseshoe magnet to revolve in front of the ends of a double induction coil. This was constructed by Pixii, in 1832, and was improved by Saxton, and afterwards by Clarke, who revolved the coil instead of the magnet.

Very large magneto-electric machines have been made, notably those used in some of the light-houses in France. They were of the type known as the Alliance machines, employing fifty or sixty permanent horseshoe magnets, each capable of sustaining sixty or seventy kilograms. The objection to magneto machines is the limit of the power and intensity of the permanent magnets employed.

It has been discovered that an electric current circulating in a wire wound spirally around a piece of soft iron, renders it strongly magnetic so long as the current passes. By increasing the number of the turns of the wire, the strength of the current, and by properly proportioning the dimensions of the coils and of the iron cores, we can obtain magnets of immense power. The Stevens Institute of Technology, at Hoboken, possesses one said to be capable of lifting several tons.

The Gramme, Siemens, Brush and Farmer-Wallace machines are what are called dynamo-electric, and are those in which electro-magnets are used instead of permanent magnets, having corresponding increase in power.

In order to obtain mechanical motion by electricity from these machines, it is necessary to reconvert the current, transformed from mechanical motion back into power.

To accomplish this, a second machine is necessary, which must be connected with the first machine by suitable conductors, and from which the power can be taken off for the purposes required. The power recovered depends on the size and kind of the machine, and the electro-motive force of the current.

Electro-motive force of a battery or machine may be defined as the power it has to overcome resistance. If we compare an electric current to a stream of water, then we may say that the electro-motive force corresponds to the volume multiplied by the head; or if E equals the electro-motive force, C the quantity, and R the pressure or head, then $E = C \times R$.

The greater the electro-motive force of the current—that is, the power to overcome resistance—the greater the effect produced on the second machine. It has usually been supposed necessary that a large quantity of electricity should be conducted from one machine to the other, and hence some have supposed electric transmission impracticable because of the great size of conductors necessary. For instance, one prominent electrician asserts that a conductor of sufficient size to transmit the power of Niagara Falls a distance of five hundred miles would require more copper than exists in the deposits of Lake Superior. Another estimates the cost at $60 per lineal foot. A very interesting discussion relating to the above, by Messrs. Houston and Thompson, is printed in the January, 1879, number of Journal of the Franklin Institute.

We come now to the question, how high a rate of efficiency can dynamo-electric machines produce, and what percentage of the power applied to the pulley of the first of two coupled machines can be recovered at the pulley of the second machine?

Like most other machines, there is a wide limit of variation in the performances under favorable and unfavorable conditions; and even under the same conditions, different machines produce various quantities of electricity. Dr. Paget Higgs has obtained from a Siemens machine about ninety per cent. exclusive of friction. Prof. Trowbridge obtained seventy-six per cent., also with a Siemens machine, which he states to have been running below its normal speed. The veteran electrician, Moses G. Farmer, in a private letter, says: "I have obtained as high as eighty-five per cent.; others claim more; some may go as high as ninety per cent. under especially favorable conditions; but from seventy to eighty per cent. is a fair amount."

It appears that the Brush machine has given as high as eighty-seven and four-tenths per cent. The remainder of the

force is expended in driving the machine and producing local currents in different parts of the machine, which currents ultimately manifest themselves as heat, principally in the armatures in which the local currents are for the most part produced.

In order to get the best effect from an electric machine, the external and internal resistances must be equal. If the internal resistance of the machine be greater than that of the external parts, then a larger part of the current produced will be used in internal work, eventually appearing as heat in the machine. If the internal resistance be too small, the current developed will be below what might be obtained from the machine.

We are not to conclude that a machine which heats badly, when working through a small resistance, is therefore inefficient. We should first try the machine with proper external resistance interposed. In coupled machines the greatest strength of current passes through the conductor when the second machine is at rest. As soon as the machine starts, an electromotive force is developed in a direction contrary to that of the first machine, which tends to neutralize the current in the conductor. The greatest work is obtained from the second machine, when the number of revolutions per minute equals half that of the first machine. Experiment has borne out the theory in this respect.

In an admirable little work on the "Electric Transmission of Power," by Dr. Paget Higgs, is given the results of a series of experiments on machines running at different speeds, with the result, when the first machine made eleven hundred revolutions per minute the maximum effect was obtained, when the second machine made five hundred and one revolutions a minute in one series of trials, and six hundred and twenty-five in another. Also when the first machine made fourteen hundred revolutions, and the second six hundred and ninety-one, the maximum per cent. was obtained. These per cents. were thirty-nine, forty-five and forty-nine, respectively.

Let us now examine briefly some instances of the actual employment of electricity as a means of transmission of power. On May 26, 1879, a field was plowed at Sermaize, in France, by means of power transmitted four hundred and six hundred meters.

At the Berlin Exposition, 1879, there was in operation a railroad three hundred meters long, run by electricity furnished by a machine working in the large hall. This distance was traversed in two minutes by a train consisting of a locomotive and three wagons, in each of which six persons could be accommodated.

Sir William Thompson transmitted eight or ten horse powers more than a mile by an electric current.

Dr. Paget Higgs, in a letter, furnished me some interesting unpublished data which I am permitted to lay before you, as follows:

"The later experimental trials, of which I spoke to you, were concerned with much larger powers, and in transmitting ninety-eight horse powers, ten machines were at first employed; these by subsequent improvements were reduced to two at each end of the wire. The wire, of copper, was three-eighths of an inch in diameter, and was suspended on ordinary posts. The source of power was a head of water made available by means of a turbine. Our first machine was driven at nine hundred and fifty, and the second at four hundred and fifty to four hundred and sixty revolutions a minute. No return wire was used; the earth was employed to complete the circuit, but the earth plates were constructed on a somewhat novel manner. The distance, two and a quarter miles, is, I believe, the longest distance power has been transmitted at so high a percentage as forty-eight per cent. reclaimed. All measurements were by dynamometer, taken during actual running and not specially measured. The cost of machines and conductors, exclusive of the turbine, was twenty per cent. less than the estimated cost of putting in new boilers and new boiler house to work an existing steam engine. Please note that the machines and power require no attention, no stoker, no fireman, no fitter, and are lubricated about as often as an ordinary shafting. It is intended to double the power."

Finally we may sum up as follows:

1. Electrical transmission of power is always possible, and can be applied when hydraulic power, compressed air, and

wire rope transmission would be impossible

2. An efficiency of seventy-five or ninety per cent. may be counted on in the transformation of power into current.

3. About forty or fifty per cent. of the power applied to the pulley of the first machine can be recovered at that of the second.

Thus far the machines used at both ends of the line have been substantially alike. It is possible changes in them may show better results.

The ideal machine would be that in which the friction and resistance to the air are a minimum, and in which the ratio of internal work to external work is as small as possible.

As great a surface as possible in the armature should be exposed to the air, in order that the heat developed may be radiated as rapidly as possible.

THE ADULTERATION OF PORTLAND CEMENT.

From "The Building News."

One of the most important constructive agents of modern times is in danger of degradation from the insidious action of fraudulent parties anxious to secure high profits by the agency of adulteration. For more than a quarter of a century slags of various kinds have been used to mix with this cement—before an accurate knowledge of its manufacture had made much progress. The intention of such admixture had a two-fold object, one being to check the tendency of a cement of light weight to expansion, and the other to meet the advantages which the increased specific gravity of the slag secured where cement was sold by the ton. Such expedients were only adopted, however, long anterior to the general use of the testing machine, and the practice remained unchallenged until it was found that the increasing quantity of the adulterant resulted in a weakening of the tensile and compressive value of the cement, rendering its use questionable, and even dangerous. The beginning of this method of introducing foreign substances into a powder of Portland cement has had most pernicious results, for it encouraged the idea (not yet exploded) that sand and similar materials continue to be used for improper purposes; and, curiously enough, the best cements, which were made specially heavy, raised the greatest amount of suspicion and doubt. The cause is readily understood, for imperfect grinding left a large percentage of residuum incapable of reduction, which in character and appearance resembled coarse sand, reduced slag, or comminuted stoneware, although highly improper and undoubtedly fraudulent, such additions in moderate proportions were only negative in character and simply pre-occupied so much space which would have been more cheaply, if not more beneficially, absorbed by the sand of the mortar mixture. The more recent and continuing application of the slag adulterant is in some measure due to the improved quality of Portland cement now generally manufactured, which leaves a considerable margin between the presented tests and its actual strength. The distiller reduces the products from his still by an addition of water to level the spirits to the acknowledged standard; and, when he exceeds or falls short of its level or datum, arranges its price accordingly; but when the adulterant is pure and innocuous, no objection need be made. The cement-maker, who, by the liberality and ingenuity of his system of production, can readily exceed the maximum tests imposed by his customer, may also, like the distiller, reduce the strength of his powder to the standard by which he is assessed. The more economical means to adopt would be to reduce the fuel cost; but in this direction he is sometimes unable to protect himself from being under the obligation to give cement of a high specific gravity; so that, while fulfilling the conditions in one direction, he is saddled with an excess of strength or loss to him in another. A cement weighing 112 lbs. per bushel, when accurately powdered, can readily

meet the requirements of a test of 350 lbs. to the square inch; but the weight standard may be, and frequently is, 118 lbs. per bushel, so that the difference against the maker under such circumstances is exceptionally hard and unreasonable. During a period of high prices, such an anomalous condition of things does not press so hardly on the producer, but when successive competition and low prices occur, recourse is had to improper means to maintain profit. He has now reached these times, and the correspondence and discussion at present prevailing on the question of adulteration of Portland cement with slag indicates that it will, if it has not already attained dangerous proportions.

Slag produced in the iron industry is abundant in many districts of England, Wales and Scotland; but at present it may be regrarded as a worthless waste, notwithstanding the more or less successful attempts for its utilization. Its chemical value and resemblance in analysis to a good Portland cement indicates its suitability for purposes of adulterating that article, and the charge is made that it is now used extensively for that purpose. If so, in the interest and protection of the constructive profession and the public generally, a chemical test will have to be instituted to guard against the dangers of such a combination. We shall shortly state why such a course is imperatively necessary to check the use of so undesirable a compound as Portland cement and iron slag. Slags are of various kinds, according to the quality of the ores from which they are produced, and the fuel and fluxes used in the fabrication of the pig iron. The best and least objectionable, however, have, in their chemical constitution varying quantities of protoxide of iron, sulphide of calcium and magnesia, all of which are, even in moderate amounts, unsuitable; and we may say unsafe, to mix with Portland cement. In their physical characteristics slags have a strikling resemblance to Portland cement "clinker," as it comes from the kiln, and, when reduced to powder, are still more similar in color and general appearance.

In cost, however, there is a great difference in value, and hence the temptation to adulterate with slag. The majority of slags when added to the "clinker" and ground together through the millstone, induce an energetic initial set of the powder, and generally realize high tensile breakings when the briquettes are kept out of water. Under the action of water, however, but sometimes very protracted in its character, the dangerous quality of the obnoxious ingredients named becomes apparent in the gradual degradation of the sample. We could not give a better illustration of this action than is to be seen in many districts where the slag heaps are *dusting;* and, indeed, in some of the earliest mounds, so much so has this progressed as to enable the surface to be used for agricultural purposes. The soil thus produced is found to be a fertile one, for it contains the best elements of fertility in the silica, lime and alkalies. They are, however, in a concrete or mortar mixture, dangerous ingredients: and in structures (more especially in those under water) where they are employed, the same influences which degraded the slag will ultimately reduce to powder the most elaborately-fabricated mortar. The use of slag, therefore, in any form, either as a silica agent in the manufacture of Portland cement, or as an adulterant in its finished state, should be avoided, unless some preliminary treatment of purification or elimination of the obnoxious ingredients referred to has been resorted to. We have no experience of what can be done in this direction, but we think to get rid of sulphide of calcium would involve so expensive an operation, even under the most successful circumstances, as to preclude the chance of its being resorted to—at least, for the adulteration of Portland cement.

Although Portland cement has, during late years, been made of good quality, and the fortunate rivalry amongst engigineers to secure a first-class article has resulted in much good, there is still in some quarters a feeling of unrest and desire for something novel. Eccentric machines for testing, or senseless methods of treatment of the briquettes to be tested, seem to have been followed by mixtures of discordant ingredients in the belief that Portland cement can be *improved.* The simple and inexpensive character of the raw materials for producing a good cement, and the now well-known processes by and through which

it is fabricated, preclude the possibility of introducing any cheaper material capable of producing like results at equal cost. If any improvement is to be realized, it will be found in the direction of the fuel cost and machinery of reduction, which departments of this industry are having due and reasonable consideration. Chalk and clay are, indeed, so plentiful in this country that there is neither immediate nor remote chance of their rising much in value. No substitute for one or the other can be found more suitable, and the builders may well feel confident that good cement from these materials can be at all times forthcoming to meet their most extended demands.

In these remarks we have assumed that many of our readers are well acquainted with the process of making Portland cement, or, at all events, understand that there is no secret in its practice, neither is there any risk in using it when they practice the most ordinary and well understood rules for testing its quality, and guarding against its dangers when imperfect. Although this well-known cement has only been known by its general name of Portland, the rudimentary experiments which led up to its discovery may be said to have been originated by Smeaton in the middle of the last century. In his Eddystone experiments he proved that the hydraulicity of limes was due to the presence of *sand* or *clay* in their original mineral condition. Biat in France, and Pasley in this country, followed on Smeaton's lines, and Aspdin, in 1824, boldly—at least, for such a humble investigator — boldly adopted all previous knowledge, which, added to his own, culminated in his obtaining Letters Patent for the manufacture of Portland cement. The simple task of rendering ordinary limestone dust or mud hydraulic, capable of setting under water, was easily performed, and more especially when the easily-combined chalks and river clays were operated upon. The blunders committed by the early makers were due to the want of chemical knowledge, which could define with accuracy the exact proportions of the carbonate of lime (chalk) and silica and alumina (clay or river mud). The means adopted were varied, according to the experience of the operator; but usually the accurate admixture was accomplished with water as the combining vehicle. There is a gradual lessening of the hitherto objectionable amount of water so employed, and at some works on the Thames the barest quantity is now used, resulting in a considerable saving in cost. Additional machinery of a special character performs in a much less space of time the same desirable and indispensable combination or blending of the raw materials. A process which originally occupied several months in its performance can now, with equally beneficial results, be performed in a week or ten days.

Instead of differences in the mechanical tests for proving Portland cement, we should advise the adoption by all consumers and producers of one common standard, and thereby facilitate the comparison of qualities, and thus avoid differences which, under existing circumstances, are practically irreconcilable. If an approaching danger like that we have referred to should attain serious dimensions, or its progress become incapable of being checked, a new chemical test must be prepared to stultify its effects and rendor abortive the schemes of the dishonest or misguided adulterators. If we will have adulterated cement, let its sale be controlled by some such rules as regulate the sale of butter, and other articles of daily consumption.

In what we have said in reference to this question of the use of clay we wish it to be clearly understood that we do not object to its use as a constructive agent. As an aggregate, under certain controllable conditions, iron slag can be made exceedingly useful; but we protest against its being used as a binding agent of either mortar or concrete. In the concreted mass, wherein it only plays a comparatively subordinate part, the extent of danger, where it may exist, can be readily measured and controlled, which cannot be done when it is intimately incorporated with the cement.

ASPHALT AND MINERAL BITUMEN IN ENGINEERING WORKS.

By Mr. W. H. DELANO, Assoc. Inst. C. E.

From "The Building News."

Adopting the nomenclature of M. Léon Malo, which had received general sanction, the author considered asphalt as a combination of carbonate of lime and mineral bitumen produced by natural agency. Asphaltic mastic was the rock ground to powder, and mixed with a certain proportion of bitumen. Gritted asphalt mastic was asphalt mastic to which clean sharp sand had been added. Asphaltic or bituminous concrete was gritted asphalt mastic mixed when hot with dry flint or stone. Boussingault's analysis of bitumen gave C 85, H 12, O 3. It was, therefore, an oxygenated hydro-carburet, and quite distinct from the preparations of gas-tar and pitch, which were sometimes erroneously styled bitumens and asphalts. It was important that these distinctions should be borne in mind when specifying asphalt, as their disregard might lead to the employment of a material having few of the properties of the natural rock, although bearing, to the uninitiated, a strong resemblance thereto. Messrs. Hervé-Mangon and Durand-Claye, of the Ecole des Ponts et Chaussées, Paris, had supplied the author with detailed analyses of different kinds of natural asphalts, which were given in the paper, and specimens were exhibited. But beyond knowing the numerical value of the proportionate constituents, it was highly necessary that the engineer should be acquainted with their quality. Asphalts which gave almost identical analyses might, in practice, yield widely different results, if the nature of the individual components was dissimilar. Powdered limestone should be white, and soft to the touch; if rough, it probably contained iron pyrites, silicates, crystals, &c. The presence of these substances was prejudicial, and if suspected, the limestone should be subjected to a secondary analyses, directions for which were given. The proportion of bitumen to limestone in the natural asphalt should not exceed 10 per cent. for carriage-ways, indeed less than that was preferable. For this latter purpose no asphalt should be specified which had not stood the test of, at least, three hot summers and three cold winters. These precautions being taken, the author was of opinion that a well-laid surface of compressed asphalt, 2 to $2\frac{5}{8}$ inches thick, on a foundation of Portland cement concrete, 6 to 9 inches thick, was superior to all other carriage-ways. It was noiseless; hygienic, being impervious to urine and the liquids from dung; absorbed vibration; produced neither dust nor mud; was cheap, durable and easily repaired, and the old materials could be used again. The charge of slipperiness, which had been made against asphalt roadways in London, was not due to the material, but to the absence of provisions for proper scavenging. In Paris, where the asphalt was regularly scraped, washed, and swept, the complaint did not arise. In support of the assertion that climate did not affect the asphalt in London, a table of humidity was given, showing the means of six years' (1873–8) observations to be—for Paris, 80.2; for London, 81.5. The cost of washing the roadways, when done systematically and on a large scale, was much less than was generally supposed, and the advantages far more than counterbalanced the expense. The author submitted a design for a portable washing and sweeping machine for use in London. Reference was made to the cost of compressed asphalt carriage-ways. In Paris this amounted, on the average, to about 13s. per square yard on lime concrete 4 inches thick, but a thickness of 6 inches to 9 inches of Portland cement concrete was much preferable. The cost of transport of the material also exercised an important influence on the ultimate expense. Details were given of various works of asphalt paving carried out by the author, with particulars of the cost of maintenance.

The quality of absorbing vibration, which was a marked characteristic of asphalt roadways, had been taken advantage of in the application of the material for the foundations of machinery running at high speed. This was instanced in the case of a Carr's disintegrator, which, being mounted in a pit lined with bituminous concrete, was worked at 500 revolutions per minute, without sensible tremor, whereas, with the former wooden mountings on an ordinary concrete base, the vibration was excessive, and extended over a radius of 25 yards. In the Paris Exhibition of 1878, there was shown a block of bituminous concrete, weighing 45 tons, forming the foundation of a Carr's disintegrator as a flour mill, and making 1,400 revolutions a minute, a speed which would have been impracticable on an ordinary foundation. Extensive applications of the material for this purpose obtained in France, especially in connection with steam engines and steam hammers.

Another use of asphalt was for the flooring of powder magazines, where its non-spark-emitting character made it particularly valuable. It was also largely applied in France, in the form of gritted mastic for the flooring of casements in fortifications, and in its pure liquid form, for the coating of vaults and arches, where it protected the masonry from damp, and the subsequent disintegration caused by infiltration and by frost.

In conclusion, the author referred to the imitation asphalts occasionally brought forward, and by some regarded with favor on the score of cheapness. The best of these, if properly made, was as dear as the natural material, without in any degree possessing its special qualities of appearance and durability; and in no case were any of them suited as paving materials to resist heavy traffic. In Paris, the tricks of irresponsible paving contractors were many, and necessitated constant vigilance. Inferior cement was put into casks bearing established brands, and the concrete made with such cement was put down in thinner layers than was paid for. The author had even known cases where the concrete was omitted altogether, a layer of common mortar taking its place. Such foundations would insure the failure of the best asphalt, which ought to be considered only as a wearing surface or armor to the concrete. But the mode most difficult of detection was the ostentatious display, at the sight of the works, of cakes of the particular asphalt specified, while an inferior material was in the boilers. Once laid, wear alone would reveal what had taken place. From these malpractices asphalt had occasionally suffered unmerited condemnation, but the author claimed that with *bona fide* materials and workmanship, satisfactory results could always be obtained.

ON MAGNETIC CIRCUITS IN DYNAMO AND MAGNETO-ELECTRIC MACHINES.

From Papers of the Royal Society.

A LARGE amount of magnetism is retained by the soft iron cores of electro-magnets, when arranged so as to form a complete magnetic circuit; and sparks and other indications of the passage of an electric current can be obtained at the ends of the helix wires surrounding those soft iron cores, each time the masses of iron are separated, and the closed magnetic circuit opened. In order to procure a spark, the breaking of the circuit must be effected suddenly, either by a jerk, tilt, or sliding movement.

In the case of the 58 lb. magnet described in our former note, the current that is capable of causing a spark, although only momentary in duration, is found to be sufficient in quantity and intensity to magnetize a small electro-magnet, weighing, with its coils, between 5 and 6 lbs., enabling it to sustain its own weight for any indefinite length of time when suspended by its armature.

When the armature of the small magnet is placed at the distance of $\frac{1}{8}$ in. from its poles, in such a manner as to be free to move, the instant the armature of the

large magnet is suddenly tilted or slid off it darts to them, the completion of the circuit of the small magnet being signalled by a smart click. The rupture of one closed magnetic circuit is thus caused to produce another closed magnetic circuit.

But when the interval between armature and magnet, whose circuit it was intended to close, exceeded ¼ in., the former was not attracted with sufficient force to overcome the friction of the table upon which it was resting.

The mode of removing the armature from the large magnet appeared to be of no moment, but the time occupied by the removal had much influence upon the amount of magnetic force manifested in the smaller circuit. This was particularly the case if there were an interval, no matter how small, between the armature and the poles of the magnet round which the electric current was sent.

For example, if with an interval of 1-16th of an inch between the armature and the poles of the small magnet, the armature of the large magnet was slowly slid off, the magnetization of the small magnet never rose to a sufficient intensity to draw its keeper to itself, whereas, when the sliding took place rapidly, the small armature was strongly attracted as above mentioned.

The largest amount of magnetization was bestowed upon the small electro-magnet by the interaction, when it was held upright, its poles being completely covered by a closely-fitting armature. And it was also found that when thus set up in preparation for the formation of a closed magnetic circuit, the magnetization was produced by a much slower motion of the large armature than when the small magnet had its circuit partly open. When the circuit was completely closed, if the large armature were twisted off by a slow equal motion, in such a manner that both poles were uncovered at the same time, then the small magnet could be made to sustain not only its own weight (between 5 and 6 lbs.) but an additional 3 lbs. also.

During the passage of the electric current, obtained by the forcing open of the closed circuit, the fall of magnetism in the large magnet itself is checked, the direction of its magnetic polarity remaining unchanged, the current checking or opposing the fall being in the same direction as that from the battery which caused the primary magnetization. If the ends of the helix wires are not connected together, this effect is not obtained.

Electric currents, though of less intensity and quantity, can be produced in the helices of electro-magnets, without altogether breaking up the closed magnetic circuits. For instance, with the 58 lb. electro-magnet, the circuit being completely closed by its armature, and the helices being connected with a galvanometer, a very slight pull applied to the armature produces a current of electricity, giving a considerable deflection of the needle in the same direction as the battery current; and the stronger the pull the greater the deflection of the galvanometer needle, up to the point at which the magnet is lifted from the ground, after which no further motion of the needle is produced, unless the magnet is subjected to additional strain. Thus, hanging a 4lb. weight upon the uplifted magnet produced deflections in the same direction as the pull on the armature, and on removal of the weight, produced reverse deflections.

Trying the same set of experiments with a very small electro-magnet, so that we might proceed to absolute rupture of the closed magnetic circuit without danger to the galvanometer, we found that the addition of successive weights to the magnet, while hanging suspended by its armature, produced successive deflections of the galvanometer, the needle coming to rest at zero after each addition, as in the case of the large magnet.

When the maximum weight which the magnet was capable of sustaining was reached, and a real movement of the armature commenced, the induced current in the helix of the electro-magnet was very greatly increased by the addition of even the smallest weight.

From these experiments it may be inferred that in like manner as the passage of an electric current round a bar of iron produces elongation of the bar, so the elongation of the bar produces in its turn an electric current in the helix, which tends to strengthen the magnetization. It also appears that a magnet is absolutely stronger under tension than when at rest.

On the other hand, pressure on the armature, either continuous, or sudden and momentary (a blow, for example), causes an electric current in the helices in the opposite direction to original magnetization, or, in other words, against magnetization; tending thereby to weaken the power of the magnet.

The 58 lb. magnet in closed circuit was hung by its armature, and on afterwards connecting its helices with the galvanometer no current could be detected, but on lowering it until it rested with its whole weight on the ground, a current, in the direction of demagnetization, was produced giving a deflection of 15°. In the same way, a current in the direction of magnetization was obtained, giving a deflection of 15°, by the application of sufficient strain to lift the magnet off the ground, and this result was invariable. The degree of swing, however, depended upon the rapidity with which the magnet was either raised or lowered.

It may be remarked that whereas any very slight application of force by pulling on the armature was sufficient to cause a current in the helices giving a deflection of 5° to 10° of the galvanometer needle, a great amount of pressure is necessary to produce a similar deflection. A slight pull with the finger and thumb in the one case was equal to the pressure of a hundred weight in the other.

By the momentary removal of the armature, the closed magnetic circuit is broken, and though by its immediate restoration a new closed circuit is formed, nevertheless the tension on the molecules of iron by the magnetic stress is very greatly reduced. Under these conditions, a very slight pressure upon the armature produces a great swing of the needle, whilst a pull produces scarcely any effect at all, until actual movement of the armature takes place.

If the pressure on the armature is great and continuous, a point is soon reached at which a slight pressure is no longer effective.

The effects produced are somewhat different if pressure is applied unequally. For instance:—a weight of 7 lbs. placed on the armature over the north pole of the 58 lb. magnet caused a current in the helices giving a deflection of 20° at the galvanometer. The same weight on the south pole gave the same deflection in the opposite direction. Pressure of the hand produced like swings of the needle, proportionate to the force used, and the amount of swing can be easily controlled, and the needle brought to rest by judicious pressure on either pole of the magnet.

If a lateral pressure be applied to one side of the armature between the poles, and the needle swings, say 50°, on removal of the pressure, a current is produced in the opposite direction, and the reverse swing, in place of being 5°, will be 8°, and so on, in proportion to the amount of force made use of.

With the small magnets, pressure gave no recognizable current without actual movement of the armatures.

Under certain circumstances, the attractive force of electro-magnets in closed magnetic circuit is found to increase with lapse of time. For example:—A small U-shaped electro-magnet, with limbs 6 in. long, having a core of $\frac{3}{4}$ in. iron, and helices consisting of four layers of No. 16 covered copper wire, when excited by four Bunsen cells, supported as an armature a similar U-shaped iron bar, but without a helix upon it, this latter remained firmly attached after the voltaic current had ceased, but the hanging on to it of an additional weight of 3 lbs. 6 ozs. instantly wrenched it away from the electro-magnet, and broke the closed magnetic circuit.

The magnet was then re-excited, the armature being fixed to the electro-magnet by being held in contact with the poles whilst an electric current, of a few seconds' duration, passed through the circulating wire. In place of immediately attempting to add any additional weight, the two iron Us were left hanging face to face, in the form of a link of a chain, for twenty-four hours, at the end of which time the weight of 3 lbs. 6 ozs. was hung on and sustained. Forty-eight hours later, an additional weight of 3 lbs. 10 ozs. was carefully added, making in all 7 lbs. sustained. Twelve hours afterwards 1 lb. more was added, bringing up the entire weight to 8 lbs. beyond that of the armature; this was suffered to remain for five days, when the system was taken to pieces.

On a subsequent occasion, the same magnet sustained an entire weight of 10

lbs. beyond that of the U-shaped armature, the weight sustained being reached by beginning with an amount well within the sustaining power of the electro-magnet wire in closed circuit, and increasing it by small additions made with intervening intervals of time varying from twelve hours to several days.

Another, and smaller U magnet was also experimented on; this weighed, with its coils, 3 lbs. 6 ozs. Its armature was a strip of soft iron, completely covering the poles, and having a hook in the center, to which weights could be easily attached.

This electro-magnet was excited by the passage, for a few seconds, of the current from two one-pint bichromate cells. On breaking battery contact, the armature failed to sustain 4 lbs. The electric current was again sent round the electro-magnet, and the armature was pressed against the poles, being carefully adjusted so as to cover them completely, and at the same time to place the hook precisely in the center, so that the pull should be fair and equal when a weight was hung upon it. By this careful manipulation, on breaking contact with the bichromate cells, the closed magnetic circuit was found capable of sustaining the 4 lb weight.

By successive additions of two oz. weights, made at intervals of a few minutes, the weight hanging to the armature was raised to 5 lbs., after which the attempted addition of 2 ozs. caused the disruption of the system.

The experiment was repeated under similar conditions, but with slightly extended intervals of time between the additions of the 2 ozs. weights. The magnet in closed circuit was made to hold 4 lbs., 4¼ lbs., 4½ lbs., 4 lbs. 14 ozs., 5 lbs. 2 ozs., the time taken in all for the successive additions being ten minutes. The system was then left for 12 hours, when, by additions of 4 ozs. at intervals of a few minutes, the weight sustained was increased to 6 lbs. 4 ozs. Eleven hours later this was further increased to 7 lbs. 6 ozs., and two hours afterwards to 8 lbs. 2 ozs.

A still smaller electro-magnet, weighing, with its coils, 5 ozs., and having an armature consisting of a very thin slip of soft iron, when excited by one of the bichromate cells, could not be made, when in closed circuit, to sustain 1¼ lbs. at the moment of breaking the voltaic circuit. It, however, sustained 1 lb. with ease. The latter weight was, therefore, suspended, and the cell wires removed after the closed magnetic circuit was completed. By successive additions of 2 oz. weights, at short intervals of time (five minutes to ten minutes each), this small magnet could be made to sustain 2 lbs 2 ozs., but the addition of 1 oz. beyond this weight at once separated the armature and magnet. It was thought that a longer interval of time should, as in the former instances, enable the magnet to sustain a still greater weight. It was, therefore, brought into closed circuit, as before, and made to sustain 2 lbs. 2 ozs in the manner just related, and was thus left for twelve hours. Successive additions of 2 ozs. were then made to the hanging weight, until it reached 2 lbs. 14 ozs. Twenty-four hours afterwards 4 ozs. more were added, bringing the entire weight suspended to 50 ozs.

This small, soft iron magnet which, at the instant the voltaic current was withdrawn, was totally unable to sustain five times its own weight, was thus, by gradual growth of its magnetic force, enabled to hold ten times its own weight.

In the course of these experiments it was remarked that the longer the period the soft iron remained in closed magnetic circuit, the more magnetically ductile did its molecules appear to become. An electro-magnet, which had been for a few days in closed circuit, could be, after rupture of the circuit, made to sustain weights, in a fresh closed circuit, at much shorter intervals of time than if it was magnetized, after being for some time with its poles uncovered. The direction of the battery current with reference to the residual magnetism of the electro-magnets appeared to be of no moment. A magnet which had been left for some time with its poles uncovered had less residual magnetism after a momentary current had passed through its helices, than another magnet which had been in active closed circuit, even if the battery current had, in the latter case, to overcome a considerable amount of residual magnetism.

We found, moreover, that soft iron magnets retain their residual magnetism longer, and are capable of acquiring in-

creased magnetization much more rapidly after having been bearing weights (thereby keeping the iron in a state of strain), than if they have been left in their normal condition and without bearing any weight at all.

The conditions under which the closed magnetic circuit retains its force are not yet clearly established.

With the 58 lb. magnet a succession of gentle taps struck vertically with a wooden mallet upon the center of the armature, while resting on the magnet in closed circuit, in a very few moments completely dissipated the magnetic force so far as the sustaining power of the magnet was concerned.

Removal of any portion of the weight suspended to the armature of a magnet hung up in closed circuit likewise tends to dissipate the force of the circuit. For example: Half an hour after the removal of a weight of 10 lbs., which had been suspended to the armature of a U magnet for 21 days, the armature fell off on receiving a slight touch. In another experiment, a U magnet, which was capable of sustaining 7 lbs., and which had actually been suspending 4 lbs., was left for two months with the armature on only, the weight having been removed; at the end of that time a very slight shake was sufficient to cause the armature to fall of. Many other examples might be quoted to show that release from strain diminishes the magnetic force of the circuit.

In these experiments, in which the closed magnetic circuits had given way, the soft iron had been in a state of strain from which it had been released by the removal of the suspended weights. But when no weights were hung upon the armature, and the iron had never been in a state of magnetic tension, the closed magnetic circuit, so far from diminishing, increased in force. The 58 lb. magnet was excited with a voltaic current so feeble, that although the magnet could be lifted by the armature in closed circuit, yet great care was necessary that the lift should be exactly vertical; and very little force was required to slide the armature off the poles. After the lapse of a month, the armature was so firmly held that the utmost exertion of manual force could not stir it by a sliding movement, and the whole magnet could be raised from the ground, even if tilted as much as 15° from the perpendicular.

The magnetism of the closed circuit of the 58 lb. magnet disappears, after repeated up and down movements of either one or both of its helices, provided the ends of the helix wires are connected together, either singly, in two separate circuits, or together, in one continuous circuit. Every up or down movement of either of the helices produces currents in the wires either for or against magnetization, which currents apparently so disturb the molecules of the iron that the fixity of their original magnetic direction is lost.

In like manner as the movements of the armature, or the increased or diminished tension of the iron, produce currents of electricity in the helix wires surrounding the magnets, so the movements of the helices produce currents of electricity which may either magnetize or demagnetize the iron. With the 58 lb. magnet in closed circuit, the two ends of one of the helices being connected to the galvanometer, and the two ends of the other helix being connected with each other, the latter helix is moved towards the armature, a current is produced in the galvanometer helix which shows a fall of magnetization. On moving the same helix away from the armature, a current is produced in the direction of magnetization.

In another experiment, 30 yards of No. 16 covered copper wire, with its ends connected together, and so coiled that it could be moved freely from pole to pole over the armature, was placed on one limb of the 58 lb. magnet and the closed circuit established. Both helices were then brought into continuous circuit through the galvanometer.

On movement of the coil of wire from south limb to the north limb of the magnet, a current was produced showing an increase of magnetization. On moving the coil in the opposite direction, *i. e.*, over the north limb pole, and on to the south one, the current is reversed, and is in a direction which would cause demagnetization.

It appears, therefore, that any interference with the lines of force about a magnetic circuit means an interference with the magnetic circuit itself, and

points to the possibility of building up magnetic force of magnets by the mere movement of wires in these lines of force, though the coils moved need not of necessity be connected with the helices surrounding the magnets.

BRIEF ACCOUNT OF THE WOOSUNG RAILWAY.

By RICHARD CHRISTOPHER RAPIER, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

For many years engineers have been anxious to see a beginning of railways in China. Any success in this direction would not only open an important field of engineering labor, but would also greatly promote economical intercourse with that country. Many attempts have been made; but difficulty has always arisen from the unwillingness of the local authorities to sanction the proposed works, and from the reluctance of the Central Government to interfere with the responsibilities of its Viceroys. After much patient waiting, Messrs. Jardine, Matheson & Co., of Shanghai, and their friends, succeeded in acquiring a strip of land for a distance of about nine miles, from Shanghai to Woosung. As they possessed no compulsory powers, this was, of course, a costly proceeding, and the funds at the disposal of the committee were nearly exhausted in the purchase of the necessary land and graves. Still it was felt that the effort should not be abandoned without trial, and a small company was formed under the title of the Woosung Road Company with the intention of constructing a road, tramroad, or railroad, as opportunity might offer. In the course of the year 1875 the author submitted to Messrs. Jardine, Matheson & Co. an estimate for a railway on a small scale, which could be carried out at comparatively little further outlay, in addition to that which had already been incurred. It was also thought that, as it was doubtful how far the opposition to railways might extend, and what character it might assume, it would be advantageous for the first railway to be of moderate proportions. With this view, a very small engine was proposed to be sent in the first instance. In anticipation of some opening occurring in China, an engine had been specially built by Messrs. Ransomes and Rapier, of Ipswich. It weighed about 30 cwt. in working order, and it easily maintained a speed of 15 or 20 miles an hour. It was intended that if this little engine were not objected to, it should be immediately followed by others of eight or ten tons weight.

A contract was now entered into between the Woosung Road Company and Mr. John Dixon, Assoc. M. Inst. C.E., to complete and equip a railway on the basis of the estimate above referred to, Mr. Dixon agreeing to take a large part of his payment in shares in the undertaking. The materials were sent out in October, 1875, and arrived at the beginning of the following year.

An embankment about eight feet high, had been constructed along nearly the whole length of the line, in order to place the railway above flood level. This embankment had been made, from time to time, as the land was purchased, so as to prevent the previous owners resuming cultivation and possession.

The laying of the permanent way was commenced in January, 1876. The rails were of the Vignoles section, weighing 26 lbs. per yard, and were laid on cross sleepers, the gauge of the line being 2 feet 6 inches. The gauge was purposely fixed thus narrow, partly for economy, and partly to ensure the thorough consideration of the gauge question at the next stage of railway making. For a populous country like China, everyone concerned was in favor of the gauge of four feet 8½ inches, but funds did not admit of its adoption for the experimental attempt.

There were about twenty small wooden bridges on the line over narrow creeks, but no works of importance. The chief item of expense was the ballast. This had to be brought a distance of about 70 miles in boats, at a cost of about 5s. per cubic yard.

The little engine began to run on the

14th of February, 1876, and was received by the Chinese with enthusiasm. There were frequently as many as ten thousand visitors in a single day to see it at work. It is noteworthy that the news of this favorable reception reached London the same evening.

There seemed now to be no likelihood of opposition on the part of the people, and the completion of the line with its permanent engines and rolling stock was pushed forward as rapidly as possible. The first four miles were opened for public traffic on the 3d of July, 1876, and the whole line was completed in August, but was not opened until the 1st of December, of that year. Of the permanent engines, two weighed nine tons, and one thirteen tons, in working order. The rolling stock consisted of two first-class, two second-class, and eight third-class carriages, each accommodating about twenty-five passengers. It frequently happened, however, that the carriages had double their proper complement of passengers without any accident occurring. Indeed, during the whole working of the railway there was no accident to life or limb, except in the case of one man who committed suicide; and no accident to property, excepting that a spark from an engine once caused damage to the extent of £ 90.

The daily service of trains consisted of seven each way, performing the distance of nine miles in thirty-five minutes, with two intermediate stoppages. The first-class fare was one dollar, the second-class half a dollar, and the third-class one-sixth of a dollar for a single ticket. Nearly all the passengers traveled third-class, there being only one first-class passenger and two second-class passengers to eighty third-class. The number of passengers per train averaged about one hundred, and frequently exceeded three hundred. The station masters, drivers and guards were Englishmen. The booking-clerks, firemen and platelayers were Chinese. They were very tractable, and discharged their duties with efficiency and success.

A principal objection offered to railways in China has always been an alleged fear of depreciation of property near the line, owing to the disturbance of the "spirits of the air and of the earth." The only effect this railway had on property was the usual one, to cause a great increase in the market value of land and houses near it. The village of Kungwan, the principal intermediate station, experienced advantages perceptible at every turn. Besides the more substantial evidences of prosperity, there was at the stations a constant stand of wheelbarrows, just like an English cab-rank. Boatmen also obtained greatly increased occupation. It was satisfactory that so practical an answer was at once given to the principal objections which have been urged against railways in China.

The railway was in itself highly successful, being freely used by all classes of the community. There can be little doubt that the experiment would have been continued, had it not been for the untoward dispute between the British and Chinese Governments with reference to the unfortunate murder of Mr. Margary. This dispute gave the Chinese authorities an opportunity of alleging a grievance in the matter of the railway. The difficulty was eventually settled by the suggestion of Li Hung Chang, that the Chinese Government should purchase the undertaking. As that statesman was known to hold very enlightened views, this proposition was acceded to by the company. It was, however, exceedingly distasteful to the Governor of the province, who had to carry out and complete the arrangements. The purchase sum for the railway was fixed at 285,000 taels, or about £ 78,000 sterling, so as just to cover the outlay made by the company, and the final instalment was paid in the month of October, 1877.

In the meantime, Ting Futai, the Governor of Formosa, had expressed a desire to begin railway work in that island. The Governor of Nankin therefore availed himself of this opportunity to get rid of the railway of which he was now the possessor, but which he did not wish to keep. Every effort was made to avert so retrograde a step. His Excellency Kuo Sung Tao, the Chinese Minister in London, made representations on the subject, which were also indorsed by the British Government; but all was to no purpose. The railway was at the mercy of the Governor of Nankin, who was annoyed at having been obliged to arrange for its purchase, on behalf of his Government, against his will. He ordered that

the whole of the materials and plant should be sent by ship to the Island of Formosa. The shipment was carried out, but Ting Futai did not know that skilled engineers are a necessary part of any railway enterprise, and so no arrangements were made for any of the staff of the line to accompany the plant. Consequently the materials and machinery were landed in such a careless and negligent manner, that it is scarcely anticipated they can prove of any service.

There were about eighty shareholders in the undertaking, of whom about forty were Chinese, but the funds were chiefly found by the English subscribers. Mr. G. J. Morrison, M. Inst. C.E., was the resident engineer, and Mr. G. B. Bruce, M. Inst. C.E., was the honorary engineer in England.

Mr. Morrison has presented to the Library of the Institution a manuscript volume containing an authentic account of the undertaking, together with a full copy of all the correspondence which at any time passed with the Chinese authorities on the subject. This latter is of especial interest in view of the allegations which have been made to the disadvantage of the promoters of the undertaking; and it is of scarcely less interest in the glimpse which it gives of the Chinese view of such matters. This statement of all the facts affords complete evidence as to the entire *bona fides* of the company throughout. Mr. Morrison continues to reside at Shanghai, with the hope of making a substantial beginning of railways in a little time.

THE REGULATION OF THE WATERS OF THE JURA.

By C. De GRAFFENRIED, Engineer-in-Chief of the Works, Berne.

From the Proceedings of the Institution of Civil Engineers.

The country extending to the foot of the Eastern Slope of the Jura, from Entre-Roches to Solothurn, formerly presented vast stretches of marsh and of land frequently under water. These marshes and water meadows constituted, under the title of the "Grand Marais," the greater part of the country comprised between lakes Morat, Neufchâtel and Bienne, and extended also along the Orbe up stream from lake Neufchâtel to Entre-Roches, and down stream along the Thiele and the Aar as far as Solothurn. In the cantons of Berne, Fribourg, Solothurn, Neufchatel and Vaud, these occupied a superficies of about 190 square kilometers, which was either not under cultivation, or of which the meagre produce was frequently destroyed by inundations.

This state of things was the result of the irregular courses of the rivers, of the absence of any current in the Thiele between Nidau and Buren, and of the consequent elevation of the surface of the lakes. The Aar, swollen by the waters of the Sarine, covered (including its shingle banks) a large strip of land below Aarberg, and fell almost perpendicularly into the Thiele, near Meyenried. The latter river leaving lake Bienne with a lesser inclination, its waters became in consequence forced up stream against the natural current. This action of the Aar, the insufficiency and inequalities of the bed of the Thiele, and, above all, a sort of bar which existed above Brugg, had the effect of hindering the movement of the waters of lake Bienne, and therefore of those of lakes Neufchatel and Morat, and of considerably raising their level. Thus the neighboring country, saturated to a great distance from the river banks, was always maintained in a marshy state, and on the occasion of floods became a vast lake.

The object of the undertaking known as "The Regulation of the Waters of the Jura," is to improve the present condition of the rivers, particularly the Thiele and the Aar, by training their banks, and thus to lower the level in the lakes. The enterprise consists of—

1st. The derivation of the Aar from Aarberg on Lake Bienne by a canal from the Rappenfluh to Hagneck.

2d. The formation of a canal of sufficient capacity to convey the united volumes of the Aar and the Thiele, from their outlet on the lake, to a point below the junction of the two rivers.

3d. The regulation of the Upper Thiele and of the Lower Broye, so as to facilitate the flow of the water from lakes Neufchatel and Morat into lake Bienne.

To this must necessarily be added the opening of canals for the drainage of the marshes.

The main feature of the project, which is due to Colonel La Nicca, of Coire, is to constitute lake Bienne the regulator of the river Aar, to make it the receptacle of any gravel that may be brought down, and at the same time to neutralize the effect of the junction of this river with the Thiele at Meyenried.

The estimated cost of the works is as follows:

1. Canal from Aarberg to Hagneck..	£ 148,000
2. " Naden-Büren.....	196,000
3. " Upper Thiele.....	58,400
4. " Broye.....	29,600
5. Works between Büren and Solothurn.....	41,200
6. Administration, engineering and contingencies.....	86,800
	£ 560,000

The Swiss confederation contributes £ 200,000 to this amount. The remainder is furnished by subsidies from the Cantons interested, equal to the increased value of the land.

The Aarberg-Hagneck canal, which will carry the waters of the Aar to lake Bienne will be 5.15 miles long, with a fall of 1.4 in 1,000 to the Hagneck cutting, 4½ miles distant, and a fall of 3.75 in 1,000 for the remainder of its course. The canal will accommodate a maximum discharge of from 38,000 to 43,000 cubic feet per second. The width is 196 feet at the bottom for the greater part of the length, contracted to 131 feet in that part having a fall of 3.75 per thousand. The depth varies between 20 and 23 feet. The banks of the canal have a slope of 3 to 2, and are pitched with limestone, laid dry, and having a toe of the same material. This system of defense obtains for a length of 2.4 miles where the canal traverses compact travel. In the marshes, where the soil is turf on clay, the banks were first consolidated by a layer of gravel, and bundles of fascines, 2 feet 7 inches in diameter, were laid along the bottom before depositing the stone pitching. At a distance of 52 feet beyond each bank of the canal there is a secondary bank 20 feet wide at the top, which is elevated 24½ feet above the bottom of the canal, that is to say, out of reach, by 3 feet, of the highest known floods. These secondary embankments form at the same time occupation roads.

The most important work is the cutting through the Colline de Hagneck, which divides the marshes of lake Bienne. This cutting, the sides of which are in some parts nearly 100 feet high, will involve, for a distance of only 984 yards, 1,262,600 cubic yards of excavation in hard rocky marl. The total excavation for the Aarberg-Hagneck canal is 5,035,800 cubic yards. Except in the case of the Hagneck cutting, which had to be almost entirely made by artificial means, only a longitudinal trench (cunette), from 23 to 26 feet wide, cubing 2,082,800 yards, has been excavated. The erosive action of the water is counted upon to accomplish the remainder. To regulate this anticipated erosion, a weir has been constructed at the head of the canal near Aarberg, provided with sluices, allowing of the discharge of the desired volume of water. The result of the erosion in 1879 was very satisfactory. The water transported into lake Bienne 235,440 cubic yards, effecting a corresponding increase in the dimensions of the canal.

The Nidau-Buren outlet canal from lake Bienne is 7½ miles long, and has a fall of 1 in 5,000. The cross section is trapezoidal, the width at the bottom being 216½ feet, and the height ranging from 20 to 23 feet. The banks are inclined at 2 to 1. They are defended by dry stone pitching to the level of ordinary floods; above the slopes are wattled and grassed. This cut having considerable capacity, and the surrounding country being well elevated, there is no necessity for secondary banks as in the case of the Hagneck canal. The excavation amounts to 5,091,328 cubic yards, of which rather less than two-thirds have been removed by steam dredgers, and the remainder by hand labor and natural erosion, the latter action being only counted upon to the extent of 147,800 cubic yards.

The regulation of the Upper Thiele and of the Lower Broye consists only of training, deepening, and cleaning out the beds of those rivers, so as to allow lakes Neufchatel and Morat to participate in

the lowering of the level produced in lake Bienne by the construction of the Nidau-Buren and Hagneck canals.

The Upper Thiele has a length of 4½ miles, and a uniform fall of 0.16 in 1,000. The width, as regulated, is 98½ feet, the depth 19½ feet, and the banks are sloped at 2 to 1.

The Lower Broye, between lakes Neufchatel and Morat, is 5.2 miles long, in which distance it falls 1 foot. The width at the bottom is 52½ feet, the depth 16 feet 5 inches. At the head of the canals in the lakes moles are constructed.

These works, begun in 1869, are now (January 26, 1880) approaching completion. The Nidau-Buren canal is not yet quite finished, the last cut near Buren having still to be made. Notwithstanding this, it has already lowered the level of lake Bienne by 7.87 feet. The success of the works is therefore assured, and it only remains to complete them.

EXPLOSIVE AGENTS APPLIED TO INDUSTRIAL PURPOSES.

By Prof. ABEL, C. B., F. R. S., Assoc. Inst. C. E., &c.*

From "English Mechanic and World of Science."

THE author pointed out that, since this subject had been brought by him before the Institution in 1872, the advantages of explosives more violent in character than gunpowder for many important industrial uses had become so widely known and extensively utilized, that the supremacy of gunpowder, as the only practically useful and economical blasting agent, had for some time been a thing of the past. The greatly superior results furnished by dynamite, gun-cotton, and other explosive agents of the same class, when applied to work in which their rending and shattering action was valuable, had led to the replacement of powder by them in many directions. It had also had the effect of rendering miners more critical in regard to the quality of blasting powder, a result which has operated beneficially, not only by requiring the bestowal of greater care upon the manufacture of blasting powder, but also by leading to improvements in the nature and form of powder. An improved blasting powder of Messrs. Curtis and Harvey was referred to as one illustration of this. An account was given of the advantages attending the employment of compressed powder, in the form of the charges first devised by Messrs. Davy and Watson, and manufactured by Messrs. John Hall & Son, which were rapidly coming into extensive use, and which presented unquestionable advantages over granular powder, on the score of convenience and comparative safety, as well as of greater efficiency. Other improvements in the application of gunpowder having been referred to, the author proceeded to examine into the progress which had been made in the production and application of preparations of gun-cotton and nitro-glycerine, observing that but few of the many proposed substitutes for gunpowder, to which he had alluded in 1872, had received any important applications.

The advantages attending the employment of wet gun-cotton were described, and the manner in which its detonation was brought about was examined; the theory of the development of detonation, as distinguished from explosion, and of its transmission, being incidentally discussed. Various important technical applications of wet gun-cotton, dynamite, &c., were referred to, as illustrating the utilization of the comparatively instantaneous character of detonation. It was pointed out that the safety, power, and comparative simplicity attending the application of wet gun-cotton to the larger operations for which violent explosives were valuable, had led to its adoption for submarine mines, torpedoes and military engineering operations generally. On the other hand, compressed gun-cotton, employed either wet or dry, was now only used to a limited extent as a blasting agent, chiefly in the form of preparations sold under names by which their actual nature was disguised. Thus, a variety of nitrated gun-cotton, converted

* Abstract of a paper, before the Institution of Civil Engineers.

into compressed charges, simillar to the original compressed pure and nitrated gun-cotton, was supplied to the miner under the name of Tonite. and its employment as an efficient blasting agent was gradually extending. An account was given of the rapid progress which had been made in the application of the nitro-glycerine and Kieselguhr mixture, called Dynamite, to the exclusion of other plastic nitro-glycerine preparations. The employment of dynamite upon a large scale was illustrated by reference to the stupendous operations connected with the destruction of the reef at Hell Gate, in East River, New York, when a total of 49,915 lbs, of dynamite and other nitro-glycerine preparations was exploded in one single operation. The objections to the employment of nitro-glycerine in the pure liquid state were pointed out. Reference was made to the tendency of dynamite to freeze, and the necessity for thawing it before use, as a prolific source of fatal accidents in connection with mines and quarries, owing chiefly to the recklessness of the men, and their disregard of caution and instructions. In the course of the paper, the author referred repeatedly, and in strong terms, to the mischievous and frequently disastrous effects of misleading statements with respect to the safety of particular explosive agents, such as the absence of noxious gases in connection with their use, &c., which had, from time to time, been published and circulated in mining districts by the manufacturers and venders, and which not only engendered false ideas of safety, but also encouraged the natural tendency to disregard precautions.

An account was next given of a new class of nitro-glycerine preparations, devised by Noble, of which the so-called blasting gelatine was the type, and which presented such decided advantages over dynamite in several directions, that they had already, to an important extent, supplanted it on the Continent, and promised to extend greatly the safe and efficient application of nitro-glycerine. In giving an account of the properties of blasting gelatine, and of certain difficulties which had to be overcome in its application, the author described a series of experiments he had made with a view of increasing the relative power, &c., of the more important explosive agents. Reference was also made to useful practical results which attended investigations on the transmission of detonation to considerable distances. The paper concluded with a review of the beneficial results in connection with the manufacture, transport, storage, and use of explosive agents, which had attended the judicious application of the measures included in the Explosive Act of 1875; and with these comments, on the one hand, on the necessity for increased activity on the part of local authorities in some directions, in connection with the Act, and, on the other hand, on the danger to the public and to commercial interests, resulting from the persistent refusal of railway authorities to facilitate the legitimate transport of explosives.

THE MANUFACTURE OF PRESSED FUEL.

By E. F. LOISEAU, Philadelphia.

From Transactions of the American Institute of Mining Engineers.

In a paper on the manufacture of artificial fuel, read at the Philadelphia meeting of February, 1878, I enumerated the difficulties which I had to overcome before succeeding in the mixing of coal-dust and clay, the compressing of the same mixture, and the water-proofing of the lumps. The drying of the lumps, after leaving the press, was the remaining difficulty, and it was expected that a plan devised by Dr. Charles M. Cresson, of Philadelphia, would enable us to dry the fuel as rapidly as it was moulded, and that a continuous production could in that way be obtained.

The company was reorganized. The works were purchased by the new company at an assignee's sale, and the oven was modified, according to Dr. Cresson's plan.

Anticipating a possible failure, I had prepared a plan by which I expected to be able to demonstrate that anthracite coal-dust, mixed with pitch, could be

manufactured with our present machinery slightly modified; so that, after all, if we were compelled to give up the attempt to make fuel for domestic use, there was a possibility of succeeding in the manufacture of a good steam-fuel.

The plan suggested by Dr. Cresson for drying the pressed lumps of coal-dust, cemented with clay, did not work as well as we expected. It enabled us to dry more fuel than we did before, but it could not be made to dry more than one-half of the lumps produced by the press. The plan was abandoned, and I was authorized to experiment with coal-dust and coal-tar pitch.

The cement which is used in Europe to conglomerate coal-dust is usually dry pitch, which is prepared by separating from the tar, at a temperature of 572° Fahrenheit, the volatile matters which it contains. Some manufacturers, however, employ crude tar, others, a rich tar, which has been cleared of 25 per cent. of its volatile substances, by heating it to 392° Fahrenheit. But with common tar very weak fuels are obtained, which do not burn well, and give out a strong smell and a great deal of smoke; it is also necessary to subject them to a baking process, in order to solidify them, and to eliminate the more volatile of the materials contained. This operation of course requires a special plant, the cost of which increases sensibly the price of manufacture, without counting the products which are lost, which have an industrial value. The crude coal-tar is also very inferior to the dry pitch, which can be broken and even pulverized when cold, and be thoroughly mixed with the coal dust. This produces briquettes that give off very little smell.

The mixing of the coal-dust and pitch is usually carried on in a vertical cylinder, into which the coal-dust and pitch are charged continuously and automatically. These substances are heated gradually in the cylinder or mixer by jets of steam which are discharged upon them from all sides: they are then triturated and amalgamated by a series of blades fixed on a vertical shaft. Arriving at the bottom of the cylinder, the materials are discharged in a pasty condition, through openings, from which they are placed or conveyed to the moulds.

In order to obtain a good lump from this paste, the pressure must be at least 3,000 lbs. per square inch, and in certain cases, with hard or lean coal, it is necessary to increase this by 50 per cent. This heavy pressure is required by the nature of the paste, in order to expel the water which it contains, and to bring it to a compact condition. In European mixers the steam injected into the materials escapes with difficulty and condenses rapidly, hence the moisture in the mixture, which is only expelled by strong pressure.

When steam is injected through perforations into the materials to be mixed it loses in reality its pressure, that is, the tendency to push asunder the sides of its containing vessel; but at the same time it produces a temperature corresponding to a considerable pressure. Steam gives up first its latent heat, and then, after suffering condensation, a portion of its free heat corresponding to the difference of temperature, and the mass thus becomes continually heated. This, however, requires time, and it occurred to me that if I could dry the coal-dust first, bring the same to a certain degree of heat, and mix it with coal-tar pitch in a molten state, I would obtain more rapidly a plastic mixture which could be moulded by the same rollers used previously to mould the mixture of coal-dust and clay.

I was well aware that my mixer was not the right apparatus to mix rapidly coal-dust and melted pitch, but I had seen at work a mixer invented by Mr. August Deitz, of Philadelphia, for the mixing of sand and asphaltum for paving purposes, and I had no doubt that it could be modified to answer my purpose.

Before obtaining the means to make the required alterations in the plant, I had to demonstrate the possibility of making the fuel in this way. I made the demonstration in a very primitive way. I hired two men engaged in the tar and gravel-roofing business, and had them melt the pitch in the yard and hoist it up in buckets, from which I dipped the pitch with a gallon measure, and emptied it into the mixer. A certain quantity of coal-dust previously heated, had before this been discharged into the mixer. In the bottom of the mixer I had placed a steam pipe, 1 inch in diameter, with perforations of $\frac{1}{8}$ inch, through which I injected steam into the materials until

they were brought to a plastic condition, when I gradually discharged them into the hopper of the press, and moulded the same without difficulty.

The moulding rollers are hollow, so as to enable us to warm them by steam. As I had no steam connections made, in order to prevent the adhesion of the materials to the rollers, the moulds were lubricated by means of two tin pans, filled with water, placed underneath each roller, and in which it revolved to a certain depth.

The lumps were very hard; the demonstration seemed to be conclusive; at least it appeared so to one of our stockholders, who offered to make the required alterations at his own risk if he was allowed to try a mixer which he had devised, and which, he thought, would answer my purpose as well as Dietz's mixer. The attempt was not a successful one, and as our means were nearly exhausted, I had but a poor chance of carrying out my ideas, when another stockholder came in who approved my plans, and offered to apply them, on certain terms and conditions, which were accepted by the company.

There is a rule attributed to Bacon which says: "Begin with observation, go on with experiments, and supported by both, try to find a law and a cause." I tried my best to apply that rule. The man who is experimenting, and wants to have absolute facts to work upon, is often made to doubt his own sagacity and capability, for he must often change his course of action by reasons of deductions drawn from experiments. It so happened with me. I had carefully planned with Mr. Dietz all the details of his mixing machine, in order to adapt it for our purpose. Still I had lost sight of one essential point, and that was to keep the materials, when mixed and brought to a plastic condition, in a hot condition in a close conveyer instead of an open one, as we have now. The pitch acquires its cementing properties from 170° to 212° Fahrenheit; below 170° it loses them. When exposed to the atmosphere, the mixture chills gradually, and when the pitch coating of the particles of coal is chilled it prevents the perfect adhesion of the particles under pressure. While the pressed lumps are still warm their surfaces are smooth, and the chilled particles apparently adhere, but when the lumps are cooled, the rubbing of one lump against another sets loose the chilled particles which accumulate and create dust again in the coal pockets in the carts, and in the cellars of the customers. This defect, however, can be easily remedied by replacing the open conveyer under the mixer by a closed one, and heating the moulding rollers with steam.

In Dietz's mixer are two horizontal shafts, to which are clamped a series of blades placed at opposite angles, and which make 35 revolutions in a minute. When the materials are mixed they are dropped into the conveyer underneath, through apertures in the bottom of the mixer, which are opened and closed by means of sliding doors operated by a lever. In this conveyer, the materials are also carried forward towards the hopper of the press by blades placed at the same angle on two horizontal shafts, but they make only 3½ revolutions per minute. With this mixer a quantity of materials, weighing a little over 1,000 lbs., is mixed and brought to a plastic condition, ready to be moulded, in the short space of 2½ minutes.

The coal and the pitch are both measured, and the proportions are 9 per cent. of pitch to 91 per cent. of coal-dust.

The moulding press is composed of two rollers geared together, on the periphery of which are milled out a series of semi-oval cavities, connected with one another, in order to facilitate the dropping of the lumps from the moulds on an endless belt placed underneath.

The efficacy of moulding rollers is not accidental or arbitrary, but is governed by certain rules which may be determined on mathematical principles, if not with perfect exactitude, at least with a tolerable degree of accuracy. Moulding rollers accomplish the compression of materials more by a squeezing or bruising action. They possess the great advantage of squeezing the materials so that the feed is only a short time between the rollers. This advantage is a very important one, and it will not be surprising if rollers, as a matter of fact, are destined hereafter to play a great part in the manufacture of artificial fuel.

If we follow the materials in their passage through two rotating rollers, we

find that they begin to adhere at a certain point, depending partly on the dimensions of the rollers and partly on the size of the lump. The particles of coal coated with pitch receive no pressure at the first point of contact from the face of the rollers, but from the drawing-in action of the two revolving rollers. The squeezing pressure which is thus exerted on the materials is produced entirely by the gear of the rollers, because, through the rotating motion, the plastic mixture is drawn into a gradually decreasing compass, and must be highly compressed and moulded. This reduction takes place regularly, both rollers possessing an equal speed. The speed being equal, the product leaves the rollers in the shape given by the moulds.

If the arrangemant of the compressing rollers is such that they may be approached to one another at will, by means of springs, the first result must be a diminution of the amount of power required, in comparison with the rollers with fixed pressure. The feeding of the materials will also be more regular, and the danger of breakages from pieces of iron, stones, etc., which are often found in the coal-dust, will be avoided, the springs yielding to allow the passage of these foreign substances through the rollers. It is to be regretted that our rollers are brought together by means of screws, instead of springs.

The great difficulty is the regulating of the feed. Rollers of large diameter draw in the feed better than those of a smaller diameter. The feed ought to enter under the regulating diaphragm, along the whole length of the rollers in an even stream; still this cannot always be the case, because the stream of materials is not even. A certain friction takes place between the particles of coal and pitch, because the proportion of pressure on the particles of the feed in the middle varies from the pressure exerted on the particles next the rollers, the latter being more compressed, and sometimes crushed. The entry of the feed should, therefore, not be forced, for in this case either a portion of it will pass through the rollers not sufficiently compressed, or a stronger pressure will have to be employed, which would alter the result desired, and would produce lumps sufficiently compact to resist rough handling without breaking, but not sufficiently porous to insure free combustion, without a blast or a strong draft.

The greatest difficulties experienced in the moulding of the coal and pitch were to obtain a regular feed of materials, and to prevent the accumulation of materials which solidified in the hopper of the press. These accumulations prevented also the regular delivery of the materials between the rollers. I succeeded in overcoming these difficulties by a very simple contrivance which works perfectly.

The coal-dust is dried and heated by two sets of four revolving drums, which answer well enough in dry weather, but when the coal is very wet we have some difficulty, and we are unable to dry and warm a quantity of coal sufficient to keep the mixer and the press running. This defect, however, can be easily remedied by increasing the size of the outlets for the escape of the moisture evaporated from the coal.

The defects of the present plant could have been corrected long ago, had I had the opportunity of carrying out my ideas. Through force of circumstances I was compelled to allow others to try plans of their own. The result was expensive, unsatisfactory, and unsuccessful experiments, the legitimate outgrowth of which was disappointment, disagreement, loss of time, of money, and of production. At last, however, I was allowed to have my own way, and the result was a success, although obtained with imperfect means.

The coal was placed in the market by myself, and I introduced it from the start for domestic use. It was supposed that the smoke and the strong smell of the burning pitch would be a serious objection to its use, but by careful instructions given to the customers, the inconvenience from the smell and smoke was hardly perceptible to those who followed instructions.

While experimenting with the fuel in different heating apparatus, I ascertained that when the lumps were but half consumed, if the poker was handled roughly, the particles of coal would disintegrate, and would fall, unconsumed, through the grate-bars into the ash-pan, seemingly increasing the quantity of ashes, but in reality losing the heating power of the

unconsumed coal. This was caused when the lumps were red-hot to a depth of about a quarter of an inch. Each lump would then become, so to say, a small retort. The pitch which held the particles of coal together, in the center of the lump, would gradually be drawn through the red-hot crust of the lump, and be consumed, and when the lump itself was partly burnt, and reduced to about one-third of its volume, there was not sufficient pitch left in the nucleus to keep the particles of coal together until they were consumed.

In order to remedy this very serious defect I mixed with the anthracite coal-dust about 8 per cent. of powdered bituminous coal. The result was a better fuel, which did not disintegrate, coked in the fire, and was almost entirely consumed, leaving but a small quantity of ashes, when compared with the fuel made from anthracite without the addition of bituminous coal.

This last fuel has found a ready market. It ignites readily, lasts as long as the ordinary anthracite coal, and it does not clinker. A good many of those who have tried it do not wish any other, and they send in new orders whenever their supply is exhausted.

It has been the main object of all inventors of machinery for the manufacture of artificial fuel to obtain a large production in lumps of a small size. It is easy to obtain a large production in lumps of a large size, and no better machine has yet been devised to obtain a large production than that described by Dr. Grinshaw in the *Journal of the Franklin Institute*, of September, 1879, and which is manufactured in France, by the Société Nouvelle des Forges et Chantiers de la Méditerranée. The production of a double machine, of the smallest size, does not exceed 96 tons in 24 hours, in lumps weighing very near 3 lbs. My press will manufacture in 1 hour, 13 tons of lumps weighing only 2½ ozs. each. These lumps require no drying or baking. They are conveyed to a screen in eight minutes, and that time is sufficient to cool the lumps. They are then ready for delivery.

The pressed fuel would be much improved if the coal-dust was previously washed, and in the erection of new works it will be essential to provide washing apparatus for that purpose. The difficulty now seems to be to secure a sufficient supply of coal-dust at the shipping points; and as there is a market for pea and dust, the coal companies do not feel inclined to dispose in our favor of the dust proper, so as to enable us to manufacture a fuel which would compete with their own coal. The successful manufacture of the pressed fuel, being however, a demonstrated fact, it will evidently be in the interest of the large companies to erect machinery to utilize the coal-dust, instead of piling it up around the mines. Whether the manufacture of the pressed fuel is carried on by us or by the coal companies, the community at large will be benefited by the utilization of coal-dust, which was considered, until recently, a worthless material.

I have struggled during twelve years to obtain this result. I persevered under the most trying circumstances, having to overcome financial as well as mechanical difficulties. I am satisfied now that very little remains to be accomplished in order to make the manufacture of pressed fuel from coal-dust one of the most important industries of Pennsylvania.

From a recent consideration of the relations betwen radiation of heat and temperature, Prof. Stefan, of Vienna, is led to substitute for Dulong and Petit's known law this other: *The quantity of heat lost by radiation is proportional to the fourth power of the absolute temperature.* Dividing by six the differences of the fourth powers of the absolute temperatures of thermometer and enceinte, one gets nearly the numbers of Dulong and Petit. The law they have given agrees better with these numbers, but the numbers given by Desain and Provostaye verify better the law of Stefan. Experimenting in air, even when very rarefied, one obtains a very complex result, since recent experiments prove that air has a conducting power which remains the same, whatever its density. Prof. Stefan utilizes experiments of cooling for calculation in absolute value of the heat radiated by a body. He concludes by deducing from Pouillet's experiments the temperature of the sun, which he estimates at 5580° C.

RAILWAY CURVES.

From "The Engineer."

On the 23rd of February a train of the London Metropolitan District Railway Company got off the rails at a short distance on the down side of the junction of the lines from Turnham-green and Acton to Richmond and Kew Bridge. The train consisted of a tank engine and eight coaches, all fitted with the Westinghouse non-automatic brake. The engine was running fire-box end first. No one was seriously hurt. The right trailing wheel apparently mounted the right rail a short distance from the junction crossing. The flange ran on top of the rail for twenty yards, and then the wheel dropped outside it, smashing the chairs. One rail was broken, and five others injured. General Hutchinson, reporting on this accident to the Board of Trade, states that the line was in good order. The driver of the engine said that he was running at about twenty miles an hour when his engine left the rails; but his speed was probably somewhat greater. All the carriages followed the engine save the last. It is worth notice that as soon as the engine got off the track, the driver applied the Westinghouse brake without difficulty; but the fireman failed to put on the screw brake, as its mechanism was jammed. There is nothing remarkable about the accident itself; but the cause assigned for it by General Hutchinson deserves attention. "When I visited the scene of this accident," he writes, "there was a slight trace visible of the track of a wheel along the top of the 6 ft. rail—the high rail—of the curve of about 33½ chains radius, round which the train was running when the engine left the rails; and from the description given by the engineer of the District Railway, who was on the spot about an hour after the accident happened, this track was at the time distinctly visible from opposite the down end of the check rail of the crossing to within about 2 ft. of the first broken chairs, a distance of about 21 yards. It is therefore tolerably clear that the right front wheel of the engine mounted the right or high rail of the curve directly after the left wheel had passed beyond the check rail of the crossing, *and that its doing so was due to the want of sufficient super-elevation in the high rail to meet the speed at which the engine was traveling.* If this super-elevation amounted, as it is said to have done, to about 1½ inches at the crossing, it should have been sufficient for a speed of thirty miles an hour; and as it is not probable that the speed exceeded—even if it reached—this amount, the engine being only 150 yards from where it had to stop at Gunnersbury station, the cause of the accident was probably due to a deficiency of super-elevation in the high rail, owing, most likely, to the sleepers having sunk from the heavy rain which had fallen shortly before the accident. As it is impossible, on account of the junction crossing, to give the proper amount of super-elevation—2¼ inches instead of 1½ inches—round this curve near the crossing, it is most desirable that the check rail should be extended for some distance on each side of the crossing." There is, we think, reason to dissent from the conclusion which we have printed in italics. What that reason is we propose to set forth here.

General Hutchinson is apparently unaware of the fact that many engineers, both in this country and on the Continent, hold that the raising of the outer rail on a curve is a complete mistake; that it can do no good, and may do harm; and that they act on this principle and lay their curves without super-elevation. Again, many curves which have been originally laid with a high outer rail, have in process of time been permitted to lose their super-elevation; and it will not, indeed, be too much to say that not one curve in ten has the amount of cant dictated by theory. As the formulæ for calculating the amount of super-elevation of the outside rail on a curve are not very generally known, it may be convenient to give them here. According to the first it is assumed that on the 4 ft. 8½ inch gauge no curve of a

greater radius than 1400 ft. requires the outer rail to be raised. For sharper curves the rule runs—subtract the radius of the curve from 1400 and divide the remainder by the radius of the curve and by 1400; multiply the quotient by the width between the rails, by the square of the velocity in miles per hour, and by .782; the product is the height in inches which the outer rail must be raised. This is the formula given by Mr. Henry Law, and was, we believe, that used by Brunel; a different constant from 1400 being adopted, however, because of the width of the Great Western gauge. Pambour gives the following formula: Let R=the radius of the curve; R′= radius of the curve which the train would describe in consequence of the centrifugal force and the inclination of the tire of the wheels; e=the gauge; g=the force of gravity; V=velocity; and x, super-elevation. Then

$$x=\frac{eV^2}{g}\left(\frac{1}{R}\,\frac{1}{R'}\right) \text{ and } R'=\frac{den}{4\triangle},$$

where d=outer diameter of wheels, $\triangle$=their deviation, and $\frac{1}{n}$=the inclination of the tire. Rankine's rule is much simpler than either of the foregoing. He divides the requisite cant into two parts, one required to overcome centrifugal force, the other to resist the tendency to leave the rails caused by the circumstance that the wheels, although compelled to revolve at the same velocity, have to traverse different distances in the same time. The cant for centrifugal force=gauge$\times\frac{V^2}{15r}$ where V is the speed of the train in feet per second, and r the radius of the curve in feet. The additional cant for slip of wheels is, in inches, = 7200÷ radius in feet, for the normal gauge.

It will be seen at a glance that, according to the first of these rules, no super-elevation whatever is required on a curve of 33½ chains—that in question. It is assumed, as we have said, that at no speed up to sixty miles an hour can any super-elevation be required. Pambour's rule cannot be applied unless the inclination of the tires is known. General Hutchinson does not allude to this factor. In our opinion the want of super-elevation on such a curve had nothing to do with the engine getting off the rails, the cause for which must be sought in another direction. The engine, a bogie tank locomotive, weighs 45 tons. The bogie is of the four wheel Bissel type, and carries 10 tons 7 cwt The driving wheels are loaded with 17 tons 15 cwt., and the trailing wheels with 17 tons 2 cwt. These are very heavy loads, and well calculated to test the stability of any road. Now, it is one of the great advantages of the bogie that its leading wheels, carrying a comparatively light load, quietly compress and settle a bad road for the heavy loads which follow it. But in the case under consideration, the engine was running with the bogie last, and a weight of 17 tons was thus brought suddenly to bear on the rail. Mr. Collis, inspector of permanent way, in his evidence stated that the sleepers had been "pumping." That is to say, they worked on the ballast, which was saturated with water, and made holes for themselves in which water and mud were churned as the trains passed. It will be remembered that a very severe frost had not long departed at the time of the accident. All the facts point, we think, to a weak spot in the road giving way under the tread of a heavy engine as the cause of the disaster. The engine lurched and left the rails. The station-master at Gunnersbury actually saw the engine getting off the track, and he states that it first "gave a lurch toward the 6 ft." It is worth notice that the plates of the left hand front spring—as the engine was running—were all knocked out of the buckle, but only two—the bottom plate and the third from the bottom—were broken. It is just possible that the breaking of the spring caused the accident. In any case it seems to be clear that General Hutchinson's explanation of the occurrence is not the true one.

If we come to examine the theory involved in the raising of an outer rail on a curve, it will be seen that it involves either a total fallacy, or that a practical difficulty stands in the way of its application. Taking the last proposition first, we may prove it by pointing out that as the amount of super-elevation depends on the square of the speed at which a train is traveling, what is right for one

velocity must be wrong for all others, and accordingly instances are not wanting in which trains have actually slipped off a curve on the inside when running slowly round it because of the excessive super-elevation of the outer rail. Again, Pambour's formula cannot be used, as we have pointed out, unless the inclination of the tire is known; but in the present day the practice of coning tires has become almost extinct—that is to say, the tires are very nearly cylindrical as they leave the lathe, and in engine wheels, at all events, they very soon lose the taper altogether, a shallow groove taking its place; but if cylindrical wheels are used, it will be found that the tendency of the wheels to run in a straight line, resulting from the circumstance that they must both revolve at the same velocity, may be a much more powerful factor than centrifugal force in any attempt which the engine or carriages may make to leave the rail. The centrifugal force of a train is to its weight as $\frac{V^2}{32r}$: 1, v being the velocity in feet per second, and r the radius of the curve in feet. A locomotive engine weighing 45 tons and running at sixty miles an hour round a curve of 880 ft. or 13.3 chains radius, would have a centrifugal force of nearly 13 tons; but such a velocity is never attempted on such a curve. We may reduce the virtual velocity by one-half, either by dividing the radius or halving the actual speed of the train. In either case, as the centrifugal force varying as the square of the volocity, the force tending to carry the engine off the rails would be one-fourth of this, about 3 tons, little more than one-fifteenth of the weight of the engine; but one of each pair of wheels must be made to slip before the engine can get round the curve at all, and the resistance to slipping will equal at least one-sixth of the insistant weight on the engine. A very simple calculation, which we need not give here, will show that under some circumstances the tendency of the engine to leave the rails, because the wheels cannot revolve independently, and the axles do not radiate to the curve, is greater than the centrifugal force. Of course this has to be added to the latter, not deducted from it. It is not to be used as an argument against raising the outer rail, but in favor of the practice; but it is never taken into account at all in the formulæ we have quoted from Pambour and Law—though it is used by Rankine. In fact, both are based on the assumption that the coning of the wheels will approximately compensate for the difference between the spaces passed over in the same times by the two wheels keyed on a rigid axle, and so leave nothing but centrifugal force to be dealt with. But no such compensation really takes place, as we have pointed out, and thus it is found that the conditions under which alone the rules we have quoted will apply accurately cannot be, or at all events are not, secured in practice. It is not remarkable, therefore, that in the present day when an outer rail is raised at all, its super-elevation is fixed by a kind of rule-of-thumb table supplied to the ganger or foreman of platelayers.

Apart, however, from all questions of minute accuracy is one far more to the purpose—does the raising of an outer rail really compensate for the influence of centrifugal force? It must be clearly understood that the outer rail is *not* raised to prevent the carriages from being overturned, as is very commonly believed, but simply to keep the outer flanges from rubbing hard against, or possibly mounting, the outer rail. The theory is that a pair of wheels can traverse sideways on a pair of level rails without offering any resistance, save that caused by the adhesion of the wheels and the rails. Thus, a pair of wheels carrying, say, 12 tons, could be moved endways or in the direction of the length of the axle by a force of, say, 2 tons; and this would be true, no matter what the inclination of the tires, because as much as one wheel rose, as the flange approached the rail, the other would fall; and so the position of the center of gravity, vertically, would remain unchanged. If, however, one rail be raised, then, in order to move the wheels, a force must be applied sufficient not only to overcome the adhesion of the wheels to the rails, but enough to lift the weight as well. Now, according to Law, the proper elevation of the outer rail on a fifteen-chain curve for sixty miles an hour is 4 in., that is, very nearly one-fourteenth of the distance between the rails. Thus,

the resistance to lateral motion due to gravity in this case, will be equal to one-fourteenth of the load, or for a load of 12 tons to little more than three-fourths of a ton. It would appear that the addition of this resistance to that already offered by the adhesion of the wheels is quite unnecessary, and can in fact do no good. Practice goes to prove this proposition; in the first place, because the system of raising the outer rail has been given up, to a great extent, without causing any bad consequences; and because, in the second place, raising the outer rail does not prevent the flanges from grinding against it on curves, this grinding being induced by causes with which the super-elevation of the outer rail apparently cannot deal.

SUGGESTIONS FOR DEALING WITH THE SEWAGE OF LONDON.

By MAJOR-GEN. H. Y. D. SCOTT, C. B., F. R. S.

From the "Journal of the Society of Arts."

In compliance with a resolution passed by the Metropolitan Board of Works, in November, 1857, a report was presented to them in the following year, by Messrs. Bidder, Hawksley & Bazalgette, on the "Main Drainage of the Metropolis." In this report, the Metropolitan Board were told:

"The referees say (Report, p. 9): 'It would also appear that the black mud from the sewage contains a considerable quantity of organic matter, which is most deleterious; an immense mass of this fœtid mud has accumulated in the bed and on the banks of the river, and it is continually supplying to the water large amounts of soluble matter in a state of putrescence, and contaminating the atmosphere with most offensive emanations. It is probable that the unhealthy condition of many towns on the sea coast is caused by deposits from the sewers by mud of this character.'

"And Dr. Hofmann and Mr. Witt, at p. 7 of their report, employ the following forcible expressions:—'We cannot but emphatically insist upon it—that the formation of this mud-deposit in the river appears to us by far the most serious evil which results from the discharge of the London sewage into the river. We cannot too strongly urge this point upon public attention.' In these conclusions (say Messrs. Bidder, Hawksley, and Bazalgette) we entirely agree, and although we are not disposed to think that the whole of this noisome mud results from the deposition of sewage matter, yet we are satisfied that a real and increasing evil has its origin in this source, and that this evil has already attained such proportions as to render it essential to the well-being of the metropolis that means should be taken for its immediate and permanent abatement.

"Any person who examines the state of the Thames, especially within the tidal reaches, whether above or below the metropolis, will be satisfied that the periodical withdrawal of the water of the river from the muddy surface of its bed is, in the hot weather of summer, invariably succeeded by disagreeable emanations, only too plainly indicative of the decomposition of animal and vegetable organisms."

The main drainage of the metropolis was eventually carried out in accordance with the recommendations of this report, so far as concerned carrying all the sewage to the present main outfalls. Since 1866 these outfalls have been in full operation, and have daily discharged the raw sewage of the metropolis between the towns of Woolwich and Erith, the results being precisely such as might have been expected from the above evidence. It is true that, inasmuch as the sewage is now discharged into a larger body of water than formerly, and at a point more distant from the great center of population, the evil effects are considerably mitigated, but they are the same in kind, and in 1877, Captain Calver reported to the Conservators of the Thames, by whom he had been commissioned to examine into the question, that the Metropolitan Board of Works ought

"to be called upon at once to dredge away those portions of the accreted matter which interfere with the convenience of navigation;" and he concluded his report with an expression of his "hope that the sanitary and economical difficulties may ere long, be solved, so that the noble metropolitan river committed to their (the Conservators') care may be freed from a drawback which is impairing its convenience and usefulness, and which must continue to do so at an increasing rate without an effective remedy is speedily applied."

The Metropolitan Board, on the other hand, contend that the discharge of the raw sewage is neither damaging the river navigation nor the health of the dwellers on its banks, and Sir Joseph Bazalgette maintained at the Institution of Civil Engineers:—

"That the bed of the river had improved of late; that the improvement was not due to systematic dredging; and that the mud banks were not formed by sewage."

Without entering further into the arguments adduced on either side, it is necessary, for the basis of my reasonings, to point out clearly that the three engineers nominated by the Metropolitan Board, to report to them on the main drainage of London, laid before them the following conclusions, which, indeed, having quoted the above-recited evidence, it would have been difficult not to have arrived at, viz.:—"That mud, containing much organic matter, derived, in great measure, from the sewage discharged above low water mark, is deposited on the foreshores of the river, and there putrefies;" and, "That the condition of the stagnant mud is injurious to health." (Report, p. 95.) Further argument is not necessary to show that what was true with respect to the admission of sewage into the river at several points, situated in and about London, must be true, though to a minor degree certainly, when the sewage is thrown into the Thames by two outfalls between Woolwich and Erith. It is impossible to believe that the organic matter of the sewage will wholly lose either its tendency to deposit on the bed of the river, or to putrefy when there, whatever changes may be made in the situation of the outfalls.

The liquid and richest portion of the sewage of the 3,500,000 souls in London, who contribute to the contamination of the Thames, is, in the opinion of all chemists, after its admixture with water, irrecoverable, unless it can be utilized by irrigation; while the solid suspended matters—which all the Royal Commissions on sewage disposal have, one after the other, declared to be the most injurious part of sewage impurities—if they could be recovered, and be made into portable manure, would each year be worth more than a quarter of a million of money.

That it is impossible to accomplish this object to a large extent, I hope to be able to prove to you in spite of all that has been said and written concerning the worthlessness of sewage manures. In this matter, as in many others, English people have rushed into extremes. From unbounded and childish trust in the wildest schemes, they have now ceased to put faith in any

The object of this paper is, then, to show that if the investigations and analyses of the most eminent living chemists are correct, it follows that sufficient value can be recovered from the suspended matters of metropolitan sewage, obtained by simple subsidence, and unmixed with bulky precipitating materials, to meet all the expenses of keeping such an offensive and injurious matter as solid sewage out of the Thames.

The first point for consideration will be the composition of the suspended matters of sewage, being those which I propose to utilize, and which, however deposited, whether by simple subsidence, or precipitation by chemicals, are termed "sludge."

These matters consist, 1st, of the *debris* of human fæces, mingled with the solid dejections of animals washed from stables, court-yards, and the streets, and also of the *debris* of animal and vegetable refuse from our kitchens; and 2nd, of the mud and sand scoured from the streets. The first-named constituents possess nearly the whole of the manurial value, and it is important for our inquiry to ascertain of what fertilizing elements the fæces of a population consists. By ascertaining the proportion of these elements to the organic matter associated with them, we can arrive at the value of such refuse matters as consist partly of fæcal and partly of other organic substances.

The researches of Lehmann, Wolf,

Röderer, and Eichhorn, from which tabular extracts are given by the Rivers' Pollution Commissioners (1st Report, p. 27), enable us to arrive at the composition of fæces with great certainty; and the analyses and investigations of Professor Way, carried on at Rugby, under the auspices of the Royal Commissioners on the disposal of Sewage, will, when considered in the light given by the above extracts, assist us materially in arriving at a conclusion respecting the valuable elements derived from other sources. With respect to the second class or the detritus, as this is worse than useless, it may be rightly designated as a "profligate associate," deserving no other consideration than how to lessen it, since it still further degrades the value of a manure already too poor. In Paris, where they are rapidly carrying out a system which allows the solid fæcal matters to be recovered without admixture with detritus, the contractor can afford to collect them for the fertilizing elements which they contain; but we must deal with the matters as they arrive at the outfall, in which state the worthless ingredients greatly exceed in amount those which have a value.

The tables of Röderer and Eichhorn, above alluded to, give the annual weight of the fæces of a mixed population of 100,000 persons as 64,937 cwt., consisting of 957 cwt. of nitrogen, and 1,347 cwt. of phosphates, which, reckoning the phosphate as tribasic calcic phosphate, will amount to 622 cwt. of phosphoric acid, and this assumption we may make without important error.

From these figures it would appear that the nitrogen is to the total weight of the fæces as 1 is to 67.8, or deducting the water, which constitutes three-fourths of the total weight of the fæces, the nitrogen is to the organic matter as 1 to 16.9; and the phosphoric acid is to the organic matter as 1 to 26.

At page 29 of the Third Report of the Commissioners on the distribution of sewage, are given the results of Professor Way's experiments at Rugby, extending over two and a half years, samples being taken every two hours out of the twenty-four. This series was divided into three periods, and subsequently two sets of samples were collected for five days a year later, and the ratio of the nitrogen to the organic matter, other organic refuse being now mixed with the fæces, will be found to be on the average as 1 of nitrogen to about 15 parts of organic matter.

From these results we can conclude, without much danger of error, that the sludge of the Rugby sewage, omitting all mineral detritus, is at least as rich as fæces in that most important element, nitrogen. As respects phosphoric acid, though it was otherwise at Rugby, no great difference exists in the case of London sludge, as compared with fæces, as will presently appear. From the above different observations and sets of samples, the ratio of the phosphoric acid to the organic matter was for the Rugby sludge as 1 to 31.6.

At page 47 of the Report on Metropolitan Drainage by Hofmann and Witt, an analysis is given of the insoluble as well as of the soluble portions of the sewage from Dorset-square, London, from which it appeared that the amounts of the above mentioned substances per gallon were as 1 of nitrogen to 10 of organic matter; and 1 of phosphoric acid to 14.2 of organic matter.

Again, from some samples of sludge deposits, collected hourly throughout the day at Ealing, which were very carefully analyzed by Mr. Shepheard, F.C.S., in the laboratory of Dr. Frankland, the following results were obtained as the ratio of the nitrogen and phosphoric acid to the organic matter:

$$\frac{\text{Nitrogen}}{\text{Organic matter}} = \frac{1}{26.5} \qquad \frac{\text{Phosphoric acid}}{\text{Organic matter}} = \frac{1}{27.7}$$

The foregoing sets of experiments were made under such different conditions as to time and place and length of trials, that it is somewhat difficult to arrive at a fair mean. But an unobjectionable course, perhaps, will be to compare the highest and lowest results with the composition of fæces, as given by Röderer & Eichhorn, and take care to err, if we do err, on the side opposed to that which we are endeavoring to prove. These results are as follows:

	Highest result.	Lowest result.	Mean result.	Fæces.
$\frac{\text{Nitrogen}}{\text{Organic matter}} =$	$\frac{1}{10}$	$\frac{1}{26.5}$	$\frac{1}{18.2}$	$\frac{1}{16.8}$
$\frac{\text{Phosphoric acid}}{\text{Organic matter}} =$	$\frac{1}{14.2}$	$\frac{1}{31.6}$	$\frac{1}{22.9}$	$\frac{1}{26}$

Dr. Letheby, after carefully comparing the results of his own analyses, which were very numerous, with those of Drs. Way and Voelcker, and the figures adopted by Drs. Hofmann and Witt in their investigations, came to the conclusion that in the suspended matters of town sewage the ratio of the nitrogen and the phosphoric acid to the organic matter, was—

	From excreta.	From all other refuse.	From all sources.
$\frac{\text{Nitrogen}}{\text{Organic matter}}$ =	$\frac{1}{15.5}$	$\frac{1}{36.6}$	$\frac{1}{20.6}$
$\frac{\text{Phosphoric acid}}{\text{Organic matter}}$ =	$\frac{1}{16.9}$	$\frac{1}{18.7}$	$\frac{1}{17.96}$

It would appear, from a consideration of the foregoing results, that, in different towns, owing to the influence, chiefly, of other refuse than excreta, the ratio of the fertilizing elements to the organic matters may vary considerably; but, looking to the results of the examinations of the Dorset-square sewage by Dr. Way, we may feel some confidence that the chief fertilizing elements in the London sewage sludge will be under, rather than over, stated, if we assume that—

$$\frac{\text{Nitrogen}}{\text{Organic matter}} = \frac{1}{20} \qquad \frac{\text{Phosphoric acid}}{\text{Organic matter}} = \frac{1}{25}$$

The potash is the third and only remaining valuable element found in sewage sludge, and, according to Letheby, it owes its presence entirely to the fæcal matters. Any potash present with granite detritus is not in a condition immediately available for plant life, and cannot, therefore, be reckoned as of any manurial value. From Way's analysis, the proportion of potash to organic matter in fæces is as 1 to 18.6, but, from the analyses made by Dr. Voelcker, of the precipitated sludges of four towns named in Messrs. Rawlinson & Read's report, viz: Bolton-le-Moors, Bradford, Coventry, and Leeds, all of which, excepting Coventry, are deficient in fæces, the mean ratio is—

$$\frac{\text{Potash}}{\text{Organic matter}} = \frac{1}{51}$$

and from Dr. Way's analysis of the sludge of the Dorset-square sewer, already referred to, the potash appears to have equaled the $\frac{1}{56}$ part of the organic matter; I therefore adopt the $\frac{1}{56}$ as the proportion of potash likely to be found in the metropolitan sludge. Recapitulating the results we have arrived at, we may assume then that with each part of the three fertilizers—nitrogen, phosphoric acid, and potash—there will be associated in the sewage sludge of London 20 parts, 25 parts, and 56 parts respectively, of organic matter.

We have now to see to what extent these results will be influenced by admixture with detritus—the third, and by far the most variable, as well as the most worthless, component of sludge. The detritus should on every account be excluded, as far as possible, from the sewers. At present, however, I shall limit my observations to the separation of the detritus from the more valuable organic matters when the sewage reaches the outfalls, with a view to its exclusion from the manure to be prepared from the sludge, as well as to its exclusion from the river. That this can be effected to a considerable extent will be evident from the valuable report of Captain Calver, from which I have already quoted. Captain Calver gives, at page 16 of his report, two analyses by Professor Williamson; the first stating the amount in grains per gallon of the suspended and dissolved constituents of the sewage as it issued from the northern outfall sewer; the second—the amount, similarly stated, of the suspended and dissolved constituents of the sewage as it flowed from within the apron of the reservoir, into the Thames two hours after high water, that is to say, after the velocity of the current was diminished and the sewage had deposited the heavier particles.

CONTENTS IN GRAINS PER GALLON.

Suspended Matters.	No. 1. One hour before low water.	No. 2. Two hours after high water
Organic matters.... ...	37.24	104.97
Sand.....	44.10	23.52
Iron, alumina, carbonate of lime, &c.....	26.67	23.01
Total suspended matters	108.01	151.50

For No. 1 analysis the $\frac{\text{organic matters}}{\text{mineral}} = \frac{1}{1.9}$

and

For No. 2 " " $= \frac{2.25}{1}$

The suspended matters in the sewage, when issuing from the sewer into the reservoir, contained only 34 per cent. of organic matter, and as the sewage issued from the reservoir into the river, the suspended matters contained, at one period of their flow, about 70 per cent. of organic matter. To this extent, therefore, it is manifestly possible to effect a separation of the sand from the more valuable constituents of the sewage. We require only, for the purpose of this separation, an additional reservoir for the subsidence of the lighter organic matters, after the heaviest mineral particles have been deposited in the existing reservoir.

From this second reservoir, in which I would propose that the sewage should have a period of quiescence, we might expect the sludge to show the following analysis:

Organic matter (without nitrogen)	66.50
Nitrogen	3.50
Phosphoric acid 2.80 = tribasic phosphate of lime	6.07
Potash	1.25
Sand and inert mineral matter	22.68
	100.00

a result which corresponds very nearly with the composition of the sludge at Ealing, analyzed by Mr. Shepheard, already referred to. It is a little richer in nitrogen, however, as was to have been expected from the analysis of London sludge by Professor Way.

I should here remark that different analyses of London sewage vary considerably in the proportions of the mineral and organic matters. Letheby determined that the—

$$\frac{\text{Organic matter}}{\text{Mineral matter}} = \frac{1}{1.36}$$

It would appear, however, from the analyses of London sewage given in the Rivers Pollution Commissioners' first report, to be—

$$\frac{\text{Organic matter}}{\text{Mineral matter}} = \frac{1}{1.55}$$

and Professor Williamson's analysis, from Captain Calver's report, gives—

$$\frac{\text{Organic matter}}{\text{Mineral matter}} = \frac{1}{1.9}$$

As the detritus imparts no value to the sludge, but has a contrary effect, we shall err on the safe side if we assume as Captain Calver states, that—

$$\frac{\text{Organic matter}}{\text{Mineral matter}} = \frac{1}{2}$$

And we see that by a period of repose we may easily bring the ratio to—

$$\frac{\text{Organic matter}}{\text{Mineral matter}} = \frac{2}{1}$$

We must next inquire into the values of the fertilizing ingredients of the sludge, viz., the nitrogen, calcic phosphate, and potash.

It is usual with agricultural chemists to consider the nitrogen, associated with organic matter which freely decomposes, to be as valuable as nitrogen in the form of salts of ammonia; for instance, in guano, of which about one half the nitrogen exists in the form of ammoniacal salts, and the other half as nitrogenized organic matter, which has still to pass into the form of ammonia before it becomes operative on vegetation, this latter half is considered as valuable as the first, owing to the quickness with which it changes into the ammoniacal condition. In night soil, the rapidity of its decomposition also raises its nitrogen to the rank of nitrogen in the form of salts of ammonia; but when fæcal matters have been washed with water, so that they become less liable to putrefaction, and are mixed with other nitrogenous compounds, such as hair, vegetable *débris*, &c., which do not so readily decompose, some deduction from the value of the nitrogen should be made. Accordingly, Dr. Voelcker, when analyzing different samples of sewage manures for the report on sewage disposal, by Messrs. Rawlinson & Read, whilst assigning to the nitrogen the same value as if it existed as ready formed ammonia, stated expressly that he did so in order to avoid the charge of having put too low a value upon these manures; and in speaking on the same subject at the Institution of Civil Engineers, on the 28th of March, 1876, the price of ready-formed ammonia being at that time 16s. per unit, he said that 15s. per unit would be too high for the value of nitrogen, reckoned as ammonia, before its conversion into such. Consequently, Dr. Voelcker estimated that the calculated value of nitrogen, not already converted into ammonia, should be less than the value of ready formed ammonia by at least 6½ per cent. on the value of the latter. Probably,

therefore, we may safely calculate nitrogen, before conversion, as being worth 10 per cent. less than the market value of nitrogen in ammoniacal salts, such as sulphate of ammonia. The market price of this substance is at present 20s. per unit. I will, therefore, take the nitrogen at 18s. per unit. Concerning the value of phosphoric acid, which varies considerably as it exists, in a condition either of insolubility or of solubility, I must speak at some length; this material being one I propose to add to the sludge, as I shall hereafter explain, in such a manner as to convert this ingredient into precipitated phosphate of lime. Any error in its assumed value is, therefore, of especial importance.

Dr. Voelcker made a very exhaustive series of experiments for the Royal Agricultural Society on the salubrity of phosphatic materials, and he drew the following conclusions amongst others from his investigations. "Pure and dried phosphate of lime is sparingly soluble in water."

"In a moist state and in the voluminous condition in which it is obtained by precipitation from its solution, it is about four times as soluble in water as it is after it has been dried and heated."

"The earthy phosphates in Peruvian and phosphatic guanos, still containing a good deal of organic matter, or salts of ammonia, are sufficiently soluble in water to be readily appropriated by plants." And in a letter to me, Dr. Voelcker writes; "The absence of acidity in the manure is an advantage when it is applied to land deficient in lime. There are many such soils, and on these phosphoric acid, in the shape of precipitated phosphates, produces a better effect upon vegetation than phosphoric acid in the shape of an acid soluble superphosphate for unless the acid is rapidly neutralized by the alkaline elements (notably, the lime in the soil), and precipitated in the soil, it cannot benefit the crop to which the superphosphate is applied. On the other hand, if there is a sufficient amount of lime or of other basic constituents in the land to precipitate the phosphate in superphosphate, I consider it better, as regards the distribution of phosphoric acid in the land, to apply the manure in the shape of superphosphate than in the form of precipitated phosphate. It is, moreover, impossible to distinguish by chemical means, with any degree of accuracy, precipitated from ordinary phosphate of lime, and, in consequence, purchasers of manure will regard a guaranteed percentage of phosphates, actually soluble in water, with more favor than a guaranteed percentage of insoluble phosphates which may be present in a manure, partly as precipitated, and partly as ordinary insoluble phosphates;" and he suggests that a firm should sell manure, in which precipitated phosphates occur in plenty, "without any analytical guarantee, but on an established reputation for introducing into the manure precipitated phosphates only," and thus "give the public a reasonable guarantee they are really present in a precipitated form."

It would appear, then, that Dr. Voelcker looks upon precipitated phosphates as being intrinsically little inferior, and for some soils, superior to acid superphosphate, and as far as agriculture is concerned, the precipitated might be substituted for the perfectly soluble phosphate, without appreciable loss of manurial efficacy. The difficulty lies in the analysis, and is essentially a technical one, which it is to be hoped chemists will find the way of overcoming, as precipitated phosphates are brought more into use. Indeed, there are some who are already commencing to listen to the complaints raised against such unjust valuations of manure as are given by chemists in respect of precipitated phosphates, and Mr. Sibson, in his useful little work on artificial manures, says: "The identification of this form of phosphate (reduced or precipitated phosphate), being thus often a matter of importance, I now give its approximate amount when so required; at the same time I should plainly state that I consider it distinct from soluble phosphate;" and he gives the following scale of prices per unit, for fertilizers for 1878, such prices "being intended to apply to the purchase of manures under the circumstances usually prevailing in agricultural districts, when they are supplied in bags, carriage paid, and credit given. When bought in quantities, in bulk, for ready money, or fetched from the works, of course a lower scale would apply:"

Price per unit for—	s.	d.
Soluble phosphate	4	6
Ditto in mineral superphosphates	4	0
Precipitated phosphates	3	6
Insoluble phosphate (bone or guano)	2	6
Insoluble mineral phosphate, up to 7 per cent.	1	0
Potash sulphate	3	6
Ammonia	20	0

There can be no doubt that precipitated phosphates mingled with the decomposing matters of sludge, a very putrescible substance, are, under conditions, highly favorable to solubility. As may be seen, from the investigations of Dr. Voelcker, ammoniacal salts materially increase the solubility of phosphates. And since, as is well known to chemists, phosphate of lime is easily soluble in carbonic acid, and both ammonia and carbonic acid result from the decomposition of the organic substances, there seems no reason for giving to precipitated phosphates in sludge manures a lower value than that assigned to them in Mr. Sibson's table, under the terms of sale to which that table is intended to apply. To the phosphates naturally accompanying the organic matters which the sludge contains, we should assign the same value as in the case of bone or guano.

Let us now investigate the cost of producing precipitated phosphate of lime, intermingled with sludge. I should premise that, as our object is not the preparation of a dry superphosphate, such as is manufactured for the market, a much freer supply of water is admissible than in the ordinary process of manufacture, where a dry powdery condition is essential in the finished product; a condition, moreover, somewhat difficult to obtain. The use of plenty of water much facilitates the process of solution, by enabling the acid to act more freely and perfectly on the mineral. It also does away, in great measure, with the noxious fumes evolved in ordinary superphosphate making, so that the process can be carried on without any extraordinary precautions. The mixing may be effected in strong wooden troughs, about 9 feet by 4 feet by 3 feet, pitched inside, and the dilute acid and mineral, finely powdered, should be stirred together for some minutes, until all action ceases. For one charge of a vessel such as the above, there will be required about 20 cwt. of Cambridge coprolites and 17 cwt. of brown sulphuric acid; or if mineral phosphates, with less carbonate of lime than Cambridge coprolites, be used, a little less acid is needed. Enough water should be used in order to leave the mixture in a more than semi-fluid condition, in a state in fact which admits of being readily mingled with the sludge; this sludge must have previously had mixed with it a sufficiency of milk of lime to leave the mixture alkaline after the application of the phosphoric acid.

The cost of bringing the phosphoric acid into solution, and of adding the lime, will be as follows. I obtain the proportions, excepting for the lime and the water, from Mr. Sibson's work on artificial manures, from which I have also taken the above account of a suitable mixing trough:

20 cwt.	of Cambridge coprolites, ground	£	3	5	0
17 "	of brown acid at £4 0 0.		3	8	0
5¼ "	of quicklime at 16s.		0	2	8
9 "	of water		*nil*		
Labor of mixing 2¼ tons of dry solid matters and wear and tear of troughs			0	2	4
2¼ tons of dry solid matter, containing 1,600 lbs. of tribasic phosphate of lime, cost			6	18	0

Being a little less than 2s. per unit for a material which Mr. Sibson values at 3s. 6d. per unit, delivered to the consumer in bags, carriage paid.

There would then appear to be a fair profit on the treatment of phosphatic materials, if introduced in this way, into a manure which would find a market. I shall now proceed to show that there is every probability of being able to dispose of this manure at a price approaching that at which its constituents would be valued by chemists.

Let us first, however, consider the value of the fertilizers already existing in the sludge, to which it is proposed to add lime, and subsequently a solution of superphosphate, and thus to precipitate the soluble phosphate. From the scale of prices by Mr. Sibson, given above, we shall have to deduct from the value assigned to ammonia 10 per cent., owing to the fact that in sludge that compound has no existence, nitrogen only, capable of forming it, being present. This will reduce the value to 18s. per unit, and our figures will stand thus:

66.50 organic matter (without nitrogen).....	*nil*		
3.50 nitrogen (=4.25 ammonia at 18s. per unit).....	£3	16	6
6.07 phosphate at 2s. 6d. per unit.	0	15	2
1.25 potash (=2.30 sulphate of potash at 3s. 6d. per unit).	0	8	0
22.68 sand and other minerals.....	*nil*		
100.00	£4	19	8

In order to ascertain the quantity of precipitated phosphate which should be added to the manure, let us see what proportion is necessary to give the utmost effect to the above amount of ammonia. I should here point out that phosphates have been proved to be the ingredients without which plants cannot thrive, or even live. If any of the other mineral elements found in plants are absent from a soil, the plants may become stunted, and bear a very low crop of fruit, but they pass through the cycle of life; if phosphates are absent, however, they die. "Phosphates, therefore, not only aid themselves in the nutriment of plants, but they determine the beneficial action of the other mineral ingredients;" and, as Liebig says, "the phosphoric acid insures and increases the action of the ammonia." Dr. Voelcker is of opinion that, for a manure of general purposes, the proportion of the phosphate of lime should be to the ammonia of the manure as 4 is to 1. M. Ville specifies that the phosphate of lime should be to the ammonia in ratios varying from about 4 to $1\frac{1}{8}$ to 4 to $\frac{1}{2}$, according to the nature of the crop. If we assume, therefore, Dr. Voelcker's decision to be approximately correct for a general manure, we shall have to add to the sludge about $8\frac{1}{4}$ per cent. of precipitated phosphate, after which its composition and value would stand thus. But in order to avoid any appearance of making out too good a case for my project, I will value the finished manure on a scale which can scarcely be caviled at by the most arrant unbeliever in the efficacy of sewage manure. The valuation of the ammonia is that adopted by Dr. Voelcker in Messrs. Rawlinson and Read's report, namely, 8d. per pound. This price was based on the then market price, which was unusually low.

50.15 organic matter (without nitrogen).....	*nil*		
2.64 nitrogen (=3.21 ammonia) at 15s. per unit.....	£2	8	2
4.58 phosphate associated with the sludge at 2s. per unit.....	0	9	2
8.23 precipitated phosphate (added to sludge at 2s. 6d. per unit)..	1	0	7
1.73 potash sulphate (= .9 potash at 3s. 9d. per unit).....	0	3	4
16.08 sulphate of lime, &c., from superphosphate	*nil*		
16.55 sand, &c.....	*nil*		
99.96			
Value estimated on the manure in the perfectly dry condition.....	£4	1	3

Dr. Voelcker, in reporting to Messrs. Rawlinson & Reed on samples of sludge manure submitted to him by them, says that: "It is manifestly practically wrong to estimate the money value of such bulky and poor manures by the same standard of prices at which the commercial value of guano, bone dust, sulphate of ammonia, and similar concentrated artificial manures are estimated. A more rational and correct estimate of the true value of sewage and night soil manures is attained by comparing them with ordinary farm-yard manure, and the price which is paid for the latter," and he expresses the opinion that the utmost a farmer can afford to pay for good dung of the theoretic value of 15s. per ton, if he has to cart it half a mile, would not exceed 7s. 6d., or half its estimated value.

On the other hand, he thinks that manures sell better at the value of £8 8s. per ton, than if they have a higher value. Manifestly therefore, if he is right in his view, at this price the theoretic and market values of manures should coincide. I think, indeed, I might venture to say, that he considers that they do so, even at the price of £6 per ton. Some deduction, in any case, must be made therefore, from the value at which the above estimate of the mixture of the prepared sludge and precipitated phosphate works out, and what this deduction should be may, perhaps, be best arrived at by following the course pursued by Messrs. Hofmann & Witt, to show the disadvantages of feeble manures. I may then, for simplicity's sake, suppose the one manure to have a value of £8, the other of £4, without entailing an error of any consequence. Thus:

Price of one ton of good manure at the factory	£8	0	0
Spreading	0	0	9
	£8	0	9
Price of two tons of sewage manure at factory	£8	0	0
Spreading	0	1	6
	£8	1	6
Price of one ton of good manure at factory	£8	0	0
Carriage for two miles	0	2	0
Spreading	0	0	9
	£8	2	9
Price of two tons of sewage manure at factory	£8	0	0
Carriage for two miles	0	4	0
Spreading	0	1	6
	£8	5	6
Price of one ton of good manure at factory	£8	0	0
Carriage for five miles	0	5	0
Spreading	0	0	9
	£8	5	9
Price of two tons of sewage manure at factory	£8	0	0
Carriage for five miles	0	10	0
Spreading	0	1	6
	£8	11	6

Therefore, at a distance of five miles, there is a relative disadvantage in using the sewage manure of 5s. 9d.; and, at a distance of two miles only, of 2s. 9d. If we say then, that the manure—allowing for 10 per cent. of water, which it should contain—has a value of £3 10s. per ton, we probably shall not be far from the price which would be given for it by farmers, when once they understood its merits, within a radius of four or five miles from the manufactory. In such situations as those which would be occupied by the works on either side of the river, the market would by no means be limited to a radius of five miles from them; for with water carriage, the farmers along the whole course of the river would probably draw their supplies of manure from these factories. If the sales would even cover the expenses of manufacturing the manure, as the process would be the means of keeping the most deleterious part of the sludge out of the river, it manifestly would be inexcusable to continue to throw the solids into the Thames. Let us see, then, what these expenses would be.

The first operation, when the supernatant water is drawn off from the deposit (which will consist of about nine parts of liquid to one of solid), is to add to the sludge about two-thirds per cent. of quick lime, slacked and made into milk of lime. This is effected by running the milk over it, and then stirring the compound, which will effectually deprive the sludge of noxious smell. The next step must be to mix with the limed sludge such a quantity of the prepared superphosphate as will nearly, but not quite, neutralize the lime previously added. The mixture now becomes surprisingly inodorous, considering the origin of the greater part of it; the organic matter also loses its slimy, glutinous nature; and assisted by the precipitated phosphates and the crystalline sulphate of lime, intimately incorporated with it, the compound drains and dries with comparative rapidity.

These additions will cost for materials about 16s. 6d. per ton of prepared manure, as may readily be seen by valuing the precipitated phosphate contained in it at the 2s. per unit which we found to be the cost of making it. To remove the sludge from the tanks and to dry it, including all the expenses of treatment, except the cost of building tanks, will amount to about 20s. per ton. This gives as the profit on the manure (£3 10s., less £1 16s. 6d.), £1 13s. 6d. per ton; or, say, £1 10s. per ton.

Sir Joseph Bazalgette estimated a few years since that, roughly speaking, each gallon of sewage water carries down with it 100 grains of suspended matters, and the daily discharge, Captain Calver says, is 120,000,000 gallons in dry weather. This would yield 279,225 tons of solid matter per annum, which quantity Captain Calver thinks too low an estimate. This estimate is, however, considerably higher than would follow from the analyses given by Hofmann & Witt, and by the Rivers' Pollution Commissioners. I do not think that it can be assumed that the organic matter is more than from 50,000 to 55,000 tons per annum, and if we add to this, for detritus and mineral matter, double its weight, as found in the outfall-sewage by Prof. Williamson,

we arrive at only 150,000 or 165,000 tons of solid suspended matters per annum; whereas, with Dr. Letheby's estimate, we should get no more than 116,000 tons, and from the analyses of the Rivers' Pollution Commissioners, and Drs. Hofmann & Witt, only 130,000 tons per annum. Concerning the quantity of solid organic matter in Thames sewage, we may speak with much more confidence, then, of the quantity of the solid mineral matter. Different estimates of the former vary less than 5 per cent.

Taking the lower of the above estimates, so as not to overstate my case—say, 150,000 tons—it may readily be seen from the analyses that we may reasonably hope to effect a rough separation of the deposit. Thus, in a first set of depositing tanks, we should keep back four-fifths of the heavier particles, entangling with them only a small proportion of the organic matter; and in a further set of tanks, in which the sewage would be brought to complete quiescence, we might recover four-fifths of the organic matter (or 40,000 tons), mixed with half its weight of mineral matter, making a total of 60,000 tons available for the manufacture of a manure.

To this 60,000 tons, we have to add about one-third for the phosphates, &c., mixed with them in the manufacture, giving us, as the total amount of manure, reckoned dry, 80,000 tons, or, with the moisture, which we will take at 10 per cent., say, 88,000 tons.

The question of what is to be done with the sand or silt deposit, amounting to 90,000 tons per annum, must also naturally suggest itself. What ought not to be done with it is quite certain; we ought not to cast it into the river. It may be quickly dealt with, and rendered perfectly clean, by passing it through one of Fryer's destructors, heated with waste cinders, which are now a drug in the market, and most difficult to dispose of; or it might be used for reclaiming a portion of the marshes at the expense of pumping it to some little distance, as was suggested by one of the Royal Commissions, in reference to the whole of the deposit. Surely, even this last plan would be preferable to putting it into the river and dredging it out again, and then having it still to dispose of.

If the Metropolitan Board take measures to keep out of the river the whole of the suspended matters which will deposit in tanks of a size moderate, as compared with the total volume of sewage water, they will have done much towards rendering the London sewage practically harmless, as will be readily apparent from the following statements in the reports of various Royal Commissions:—

"The chief part of the nuisance, arising from the discharge of sewage into the rivers and streams, may be obviated by simply arresting the solid matters in the liquid.

"By far the greatest part of the solid matter which is held in suspension in water is readily deposited in rivers, covering the banks with mud, permanently raising the beds, gradually destroying the scouring power and partially silting such rivers up.

"That, however, the appearance of the water may be improved after these deposits have taken place, yet the deposited matters lying in the bed of the current are under conditions favorable to putrefaction, and when the foul mud is disturbed by the prevalence of rain during floods, it sends forth its effluvia among the populations which are near, and even, in the course of the rivers, far distant."

In short, successive Royal Commissions have repeated the truths told to the Metropolitan Board by their own advisers, Messrs. Bidder, Hawksley and Bazalgette, in their report on the main drainage. They have further informed them that "covered reservoirs, of moderate size, ought to be constructed near each outlet, for the reception of the sewage water until it shall be discharged during the first hours of the descending tide, or to enable it to be defecated by lime or other chemical agent" (Report, p. 99) before admission into the river" with a view "to the realization of its fertilizing contents, if such should hereafter become commercially valuable." (Report, pp. 98 and 99.)

In the foregoing recommendation, then, I have not exceeded that which Sir Joseph Bazalgette himself thought imperative, upwards of 20 years ago, when his main drainage scheme was devised; but in order that the sludge to be used for manure may not be degraded by the

mixture with it of a bulky precipitate of carbonate of lime, I have suggested that the coarser mineral suspended matters may first be allowed to deposit in a subsidiary tank, and next, that the sewage may be given a period of greater rest in order that the suspended organic matters may separate from the liquid, and be made available for manurial purposes. Finally, I would urge upon the Metropolitan Board the importance of carrying out the recommendation of their present adviser, given when he was acting with the above gentlemen, and I would recommend them to take steps for making the defecation of the sewage perfect by precipitating it with milk of lime. Subsidence alone will not effect the perfection of clarification which the nation might fairly require, if the sewage of London is to be thrown into the noblest river they possess.

If this further treatment be undertaken it gives us another large quantity of worthless matter to be disposed of.

For by using 12 grains of lime per gallon, it would, with the carbonate of lime derived from the sewage water, and other matters thrown down, occasion a total deposit of quite 220,000 tons per annum. This I would deal with in one or the other, or all of the following methods:—

Firstly. By adding the proper proportion of clay to be obtained from the river banks with the requisite amount of milk of lime, so as to enable the deposit to be burned into Portland cement, as is now done at Burnley, under much less favorable conditions than would exist under the circumstances I have pointed out.

Secondly. By re-burning the precipitate, and using it for a fresh portion of sewage; this operation might be repeated six times, after which the calcined deposit might be used for the manure process, or it might be sold for phosphatic agricultural lime. The phosphoric acid thus recovered would be worth upwards of £20,000 per annum.

Thirdly. By selling the deposit as a top dressing for land, for which purpose farmers might be willing to give for it say 1s. per ton pumped into barges.

I have endeavored to put my suggestions to you without any exaggeration, and I now commend my estimates and figures to the attention of those interested in this question, and capable of examining into the accuracy of my deductions. The only point on which I myself see any grounds for doubt is on the question of what proportion of the detritus it is practically possible to separate from the organic matters. To effect such a separation as I have assumed, would not, I submit, be a difficult task for engineers, and the experiment could be made at a trifling expense. The cost of the tanks, if executed in concrete, would probably not exceed £100,000, and as the sale of the manure might be expected to realize £132,000 per annum, it would certainly be sufficient to cover the interest on this sum together with the expense of disposing of the sand.

I trust that the Metropolitan Board of Works will give their careful attention to these figures, and at any rate attempt to keep out of the river all that can be detained, without further taxing the rate-payers of the metropolis. The only reasonable objection that can be urged against my suggestions is that there might be a difficulty in finding a market for so large an amount of manure of a comparatively low standard. If those in authority turn a deaf ear to my arguments, I venture to hope that Parliament will intervene, and no longer bestow upon the Thames the unenviable distinction of being the only filthy river in the country.

It is stated—says *Nature*—that a new photographic process has just been discovered in Japan by an inventor whose name is not given. One of the substances employed in the manufacture of Japanese lacquer has the property of becoming almost as hard as stone under the action of light. A slab covered with this material and duly exposed behind a photographic "negative" for some twelve hours, was afterwards scraped and rubbed with spatula and brush, leaving the hardened portions raised in low relief, and capable of being used as a block for printing.

FORMULÆ FOR PILLARS.

By JOHN D. CREHORE.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

HAVING already treated this subject in an elementary manner, in numbers 118 and 132 of this ENGINEERING MAGAZINE, and finding an endorsement and further application, by Professor Ward Baldwin, in number 137, of the principle which characterizes my rational formula, I now return to the topic in order to call attention to a few points not yet sufficiently elucidated, and to adapt the formula for use within the elastic limit of material.

Prof. Baldwin referring to the Gordon formulæ, says: "There seems to have been no attempt to show that the formulæ now in general use are incorrect." Now it is plain that all properly derived *empirical* formulæ must be correct, on the average, for the experiments from which their constants were deduced, and for all like cases; and, if constants are established for each different form of cross section for different columns, the formulæ should accord with the tests. But when the Gordon formula, viz.:

$$Q=\frac{P}{S}=\frac{36000}{1+\frac{l^2}{3000h^2}} \quad . \quad . \quad (1)$$

which applies properly to wrought-iron pillars of rectangular cross section, is applied to all forms of cross section indifferently, palpable errors may be committed. And, indeed, this formula fails to discriminate between real differences in some important cases. Take, for example, an I-beam used as a strut or column, and compute its strength by formula (1); then suppose the same beam to be split through the web into two equal channels, and suppose further, the edges of the flanges united so that we have a tubular column of exactly the same area of cross section, and of the same dimensions, l and h, as before splitting. The formula will now give the same strength as before, but we all know that the tube is a much stronger column than was the I-beam, if, by splitting, the metal has not become too thin.

Hence, for this case, manufacturers, as Carnegie Brothers & Co., are obliged to introduce $r^2=\frac{I}{S}=$ the square of the least radius of gyration, in the place of h^2, and modify the constant 3,000.

Rankine's formula, viz.:

$$Q=\frac{P}{S}=\frac{36000}{1+\frac{l^2}{36000r^2}}, \quad . \quad . \quad (2)$$

seems therefore preferable to the Gordon formula for general use, although it still has the constants deduced from rectangular wrought-iron pillars.

The ease with which the Gordon formula is applied, will doubtless prolong its existence, although it cannot enter into the peculiarities of cross section as do the formulæ which are expressed in terms of the least radius of gyration, r, instead of the least diameter, h. There seems to be a growing tendency of late, in important specifications, to employ r instead of h. This indicates progress.

I have elsewhere shown that both these formulæ fail, or rather, were not intended, for short pillars where Q is actually greater than the assumed constant in the numerator of the last member.

The rational formula above referred to, is,

$$Q=\frac{P}{S}=\frac{C}{1+\frac{Cl^2}{m\pi^2 E r^2}} \quad . \quad . \quad . \quad (3)$$

where m stands for 1, 4, or about 2.28, according as we regard neither, both, or one only, of the pillars' ends fixed.

l=length of pillar.

r=least radius of gyration of cross section.

C=crushing strength of standard specimen of the material

P=breaking weight applied at the end of the pillar, and in the line of its axis before deflection.

S=area of cross section.
E=modulus of transverse elasticity.
Q=breaking weight per square inch of cross section, when all stresses are in pounds and all dimensions in inches.

Prof. Baldwin reproduces formula (3), modifies it for columns that fail on the extended side, and then proceeds to take the virtue out of the whole, in the following language:

"The fundamental hypothesis on which the above discussion depends is, that E, the modulus of elasticity, is constant. As is well known, however, this is not the case when the material is strained to near the ultimate strength, and hence it might be inferred that the general formulæ deduced on this hypothesis cannot be used to determine the ultimate strength of columns."

It is readily admitted that the value of E varies for different loads; but it is maintained that, for a given load at a given instant, E does *not* vary; and that in the "above discussion" the required load $Q=\frac{P}{S}$, was always assumed to be given in the sense of fixed in amount, at a given instant, and therefore the only variables in the fundamental equation were x and y.

To illustrate the variation of E for different loads on cast iron, I have arranged Table I from data found in Mr. Stoney's *Theory of Strains*, and in Mr. Kent's *Strength of Materials*.

This table shows the varying values of

TABLE I.—CAST IRON.

HODGKINSON'S EXPERIMENTS. Length of Bars, ... 120 inches. Cross Section, ... 1×1 square inches.						KENT'S EXPERIMENTS. 5 inches. $\pi\times(9\frac{1}{16})^2$ square inches.		
Compression.			Tension.			Tension.		
Load. Tons, per sq. inch.	Decrement of length due last ton, inches.	E in tons of 2,240 lbs. per sq. inch.	Load. Tons, per sq. inch.	Increment of length due last ton. Ins.	E Modulus of Elasticity, lbs. per sq in.	Load. Lbs. per square inch.	Extension in inches.	E in lbs. per square inch.
1	.020338	5,900	1	.01976	13,603,520	500	.0001	25,000,000
2	.021038	5,704	2	.02027	13,260,800	1,000	.0002	25,000,000
3	.021618	5,551	3	.02171	12,382,720	1,400	.0003	23,333,333
4	.021369	5,615	4	.02318	11,596,480	2,000	.0006	16,666,667
5	.021594	5,557	5	.02479	10,843,840	2,500	.0008	15,625,000
6	.021752	5,517	6	.02727	9,856,000	3,000	.0010	15,000,000
7	.021950	5,467	6.5	.02815	9,549,120	4,000	.0013	15,384,615
8	.022154	5,416				5,000	.0018	13,888,889
9	.022374	5,363				6,000	.0022	13,636,364
10	.022477	5,339				7,000	.0026	13,076,923
11	.022567	5,317				8,000	.0032	12,500,000
12	.022802	5,262				9,000	.0037	12,162,162
13	.023014	5,214				10,000	.0041	12,195,119
14	.023523	5,101				11,000	.0047	11,702,128
15	.023539	5,098				12,000	.0054	11,250,000
16	.024409	4,916				13,000	.0059	11,016,949
17	.024805	4,838				14,000	.0066	10,606,061
						15,000	.0075	10,000,000
						16,000	.0085	9,411,706
						17,000	.0092	9,239,130
						18 000	.0101	8,910,891
						19,000	.0112	8,482,143
						20,000	.0125	8,000,000
						21,000	.0140	7,500,000
						22,000	.0160	6,875,000
						23,000	.0184	6,140,218
						23,285	Broke.	

E for cast iron, in direct compression and tension, on which the values of E for transverse elasticity depend.

It is here seen that E varies not only beyond the elastic limit, but through all the values of P within this limit, so that the ordinary modulus of elasticity is simply a mean of many values. In other words, each load and each increment of load, has a unique increment of length and a unique value of E. And the case of other materials is similar, though perhaps not so marked as for cast iron

The difficulty with the rational formula (3), lies not, therefore, in the variableness of E for a given value of Q or P; but, if it has a difficulty, it lies in assigning the correct simultaneous values to C and E, and in our want of knowledge of the sufficiency of the multiplier $\frac{C-Q}{C}$, at the instant of rupture.

If, when the deflection is great, just before rupture, we conceive the whole weight, P, borne by $\frac{1}{2}$ (say) of the cross section, S, then the intensity

$$Q=\frac{P}{S}=\frac{m\pi^2 E r^2}{l^2}$$

of compression, is twice what it would be if P were distributed over the whole surface, and hence the third member must be multiplied by $2\left(\frac{C-Q}{C}\right)$ instead of $\frac{C-Q}{C}$.

TABLE II. STRENGTH OF PILLARS AT THE ELASTIC LIMIT, IN POUNDS PER SQUARE INCH OF CROSS SECTION.

K E	Wrought Iron. 12×2240=26,880 lbs. 24,000,000 "		Cast Iron. 15×2240=33,600 lbs. 12,000,000 "		Steel. 21×2240=47,040 lbs. 30,000,000 "	
Ends. m $l \div r$	Free. 1 U	Fixed. 4 U	Free. 1 U	Fixed. 4 U	Free. 1 U	Fixed. 4 U
10	26,578	26,804	32,672	33,364	46,304	46,854
20	25,127	26,578	30,175	32,672	44,229	46,305
30	24,390	26,212	26,767	31,585	41,155	45,417
40	22,749	25,713	23,110	30,175	37,506	44,229
50	20,939	25,100	19,658	28,540	33,667	42,791
60	19,084	24,390	16,623	26,767	29,926	41,155
70	17,275	23,600	14,058	24,935	26,449	39,376
80	15,572	22,749	11,933	23,110	23,324	37,506
90	14,006	21,857	10,188	21,327	20,569	35,590
100	12,591	20,939	8,757	19,658	18,171	33,668
110	11,326	20,011	7,580	18,082	16,096	31,771
120	10,205	19,084	6,607	16,623	14,308	29,926
130	9,213	18,170	5,799	15,283	12,766	28,147
140	8,337	17,275	5,122	14,058	11,435	26,449
150	7,565	16,407	4,551	12,944	10,284	24,842
160	6,883	15,571	4,006	11,933	9,283	23,324
170	6,292	14,770	3 653	11,017	8,413	21,901
180	5,748	14,006	3 297	10,188	7,652	20,570
190	5,274	13,279	2,989	9,437	6,984	19 328
200	4,853	12,591	2,721	8,757	6,396	18,164
210	4,477	11,941	2,487	8,140	5,835	17,096
220	4,140	11,327	2,281	7,580	5 413	16,096
230	3,838	10,749	2,100	7,071	5,002	15,169
240	3,567	10,205	1,938	6,607	4,634	14,308
250	3,322	9,693	1,794	6,185	4,304	13,508
260	3,100	9,212	1,665	5,799	4,007	12,765
270	2,899	8,760	1,550	5,445	3,739	12,075
280	2,716	8,337	1,446	5,122	3,496	11,435
290	2,550	7,939	1,366	4,824	3,275	10,863
300	2,397	7,565	1,266	4,551	3,075	10,283

$$\therefore \quad Q=\frac{C}{1+\frac{\frac{1}{2}Cl^2}{m\pi^2Er^2}} \quad . \quad . \quad (4)$$

Or, in general, if S_1 is that part of the pillar's cross section, actually receiving the whole compression, then the multiplier of $\frac{m\pi^2Er^2}{l^2}$ becomes $\frac{S}{S_1}\left(\frac{C-Q}{Q}\right)$, and the final value of Q is

$$Q=\frac{P}{S}=\frac{C}{1+\frac{S_1Cl^2}{m\pi^2SEr^2}}, \quad . \quad . \quad (5)$$

where C, E, P, and Q, are simultaneous, and at the instant of rupture. Now, if E varies sensibly with S_1, and if $S_1=S$ approximately, within the elastic limit, the rational formula (3) is practically correct. And that E does vary sensibly with S_1, is inferred from the accordance of results yielded by the rational formula (3) and by experiment.

May it not, therefore, be considered a point legitimately assumed in the argument which established the rational equation (3), that although E at the instant of rupture was less than E at the limit of elasticity, yet the numerator of the simple fraction in which E occurs, was also less in the same ratio, so that the coëfficient of $\left(\frac{l}{r}\right)^2$ remained constant?

In regard to the effect of passing through the different values of E as we go from cross section to cross section of a given column, in the process of integration under a given load, it seems clear that for any abnormal change in E there is also an abnormal change in R, the radius of curvature, and that, as both these changes result from the same cause, they compensate each other in the expression for the moment, $\frac{EI}{R}$; and hence, practically, the ordinary integration is not vitiated.

In order to avoid all uncertainty attending the values of C and E at the instant of rupture, I propose the formula

$$U=\frac{V}{S}=\frac{K}{1+\frac{Kl^2}{m\pi^2Er^2}}, \quad . \quad . \quad (6)$$

where E, K, V, and U are simultaneous and within the elastic limit.

V=whole load upon the pillar.

$U=\frac{V}{S}$=mean load on unit of cross section.

K=total unit strain on the compressed material.

E=modulus of transverse elasticity, as before.

Table II gives the values of U in pounds per square inch, computed from equation (6), for wrought iron, cast iron, and steel, using values assigned by Mr. Stoney for E and K, and remembering that the ends are fully fixed, or wholly free to turn.

We may take a third of the value of U for any case in Table II, as the safe working load, when the metals yield these values of K and E. But the true values of K and E should be determined for every quality of metal used.

It will be noticed in the first two series of Table I, that E is derived from the change of length due to the latest added ton, while in the last series E is computed from the total elongation due the total load, without initial strain.

ON A GENERAL FORMULA FOR THE NORMAL STRESS IN BEAMS OF ANY SHAPE.

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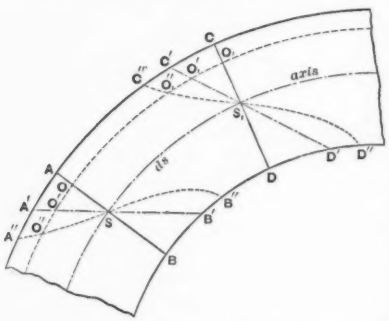
In deducing the ordinary formula for the normal stress in beams, the assumption is generally made that plane sections at right angles to the axis of the beams before the deformation remain plane after it. The investigations of de St. Venant have shown us that this assumption is in general incorrect, and have taught us how to find—in certain special cases, and under certain other assumptions—the equation to the deformed section. In fact, it is easy to see that a shearing force in the plane of the section must alter the inclination to the axis

of the beam of the element on which it acts; and if, as is generally the case, the shearing force is unequally distributed over the section, the inclinations of the various elements of that section to the axis will be different, and the section cannot remain plane. An exact determination of the deformation of a beam has, I believe, never been effected, and considering that we are as yet obliged to make assumptions regarding the nature of our materials, it may be looked upon as impossible. Moreover, even supposing us to be acquainted with the exact nature of our materials—their axis of elasticity and the relations between stress (force) and strain (deformation)—the problem of determining the exact state of stress and strain in any part of a body acted upon by given forces, presents enormous difficulties, and has never, to the best of the author's knowledge, been solved.



The equation of the deformed section, under the assumptions generally made in the higher treatment of the subject, as given by de St. Venant, Clebsch, Groshaf, Winkler and others, is found to be in most cases of a degree higher than the first, and only in some very simple cases is it of the first degree, the sections remaining planes and normal to the axis. But in some other cases the assumption that the sections remain plane, though incorrect, may lead to some correct results. Suppose, for instance, to take a simple case, that we have a beam whose axis lies in a plane, and that the outer forces also act in this plane, and that the axis of the beam remains in this plane during the deformation. Let the section AB take the position A″B″ after the bending, instead of A′B′, and let the section CD take the position C″D″, the two sections being at an infinitesimally small distance, *ds*, apart, measured along the axis. The state of normal stress between these two sections, the plane of the paper representing the plane of the axis, will be the same as though AB and CD had become A′B′ and C′D′, provided that $o'o''=o_1'o_1''$, for in that case $o''o_1''=o'o_1'$ and $o''o_1''-oo_1$, will vary directly as the distance from the line of no stress, or neutral axis. (The figure is drawn as though that line cut the axis of the beam, but this is not necessary). If the sections were deformed in this way, then the assumption of plane sections would give correct expressions for the normal stress.

The assumption that the sections remain plane amounts, in fact, as far as the strains are concerned, to the assumption that the strain (*not its intensity*) varies directly as the distance of the strained fiber from a straight line in the plane of the section, the neutral axis, but we see that the latter supposition does not require that the sections remain plane, and if it is the only supposition regarding the strains which it is necessary to make in deducing the formulæ for the stresses, then these formulæ do not require the sections to remain plane. We shall see that this is the case, and in the following demonstration we shall make the assumption that the strain on any fiber—that is, its change of length—may be expressed by an equation of the first degree, and we repeat that this does not require that the sections remain plane, although the results to which it conducts are the same as would be obtained under the latter supposition, for although this latter is only a special case of the one we make, yet the common element of both, so to speak, is the only one which we shall use in our investigation.

We may remark that in some simple cases the section after the deformation might be generated by a straight line at right angles to the plane of the axis; but that in others, for example when there is not only a shearing force in the plane of osculation, but also in a plane at right angle to it, the sections may become warped surfaces. In fact, the number of forms which they may assume, without violating the above assumption, is infinite in number.

Let us suppose a beam of any shape, with axis curved in space, and acted upon by any forces. At any section at right angles to the axis we assume three rectangular co-ordinate axes x, y, z. We take x tangent to the axis of the beam, and positive toward the right; it passes through the center of gravity o, of the section. We take the xz plane as the osculatory plane of the axis in the point o, the z axis positive upward, the y axis perpendicular to the osculatory plane in o, and positive toward the observer. We have supposed the axis of the beam given. Its determination may in many cases be difficult, for the axis may be defined as a line passing through the centers of gravity of all sections normal to it, hence a determination of the position of the sections supposes the axis already known, and the latter can in many cases only be found by a tentative process. Supposing, however, the axis known, let us consider the part of the beam to the left of the section, and apply at each element of that section the stress exerted upon it by the part of the beam to the right, which we suppose removed. Resolve all the outer forces (not the last-mentioned *stresses*) acting on the part of the beam under consideration into three forces, P_x, P_y, P_z, acting through o and parallel to the three co-ordinate axes, and three moments, M_x, M_y, M_z, acting about those axes. The part of the beam considered is in equilibrium under the action of these forces and moments, together with the stresses acting on the elements of the section. If we suppose that on an element df of the section the force kdf acts, and if we resolve this force into three rectangular components, N, S_y, S_z, acting in the direction of the axes x, y and z, respectively, and if we furthermore consider all forces positive when they act in the positive direction of the axis to which they are parallel, and all moments positive when, if viewed from the positive extremity of the axis about which they act, they are right-handed, then we shall have the following six conditions of equilibrium between the outer forces and moments, and the inner or molecular forces acting at the section:

$$\Sigma \mathrm{N}df + \mathrm{P}_x = o \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\Sigma \mathrm{S}_y\, df + \mathrm{P}_y = o \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\Sigma \mathrm{S}_z df + \mathrm{P}_z = o \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\Sigma \mathrm{S}_y df\, z + \Sigma \mathrm{S}_z df\, y + \mathrm{M}_x = o \quad . \quad (4)$$

$$\Sigma \mathrm{N}df.\, z + \mathrm{M}y = o. \quad . \quad . \quad . \quad . \quad (5)$$

$$\Sigma \mathrm{N}df.\, y + \mathrm{M}_z = o. \quad . \quad . \quad . \quad (6$$

the summation being in each case extended over the whole section. It is not the purpose of this paper to investigate the distribution of stress over the section in all its generality, but simply to find an expression for the normal stress N. We shall, therefore, pay no further attention to the forces P_y, P_z, or to the moment M_x, for they only cause shearing stresses in the plane of the section. It will also be convenient if we alter our method of designation, and consider P_x positive when it acts in the direction of the negative x axis, and M_y positive when, if viewed as above, it is left-handed. Making these changes, leaving out of account the equations (2), (3) and (4), and changing the Σ to a $\int$ we have as our conditions of equilibrium:

$$\mathrm{P}_x = \int \mathrm{N}df \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\mathrm{M}_y = \int \mathrm{N}\, z\, df \quad . \quad . \quad . \quad . \quad (5)$$

$$\mathrm{M}_z = \int \mathrm{N}\, y\, df \quad . \quad . \quad . \quad . \quad (6)$$

The first equation expresses the condition that the axial force P_x equals the total normal force acting at the section, while the last two express the equality of moments about the axes of y and z, n being positive when it acts toward the right. A positive N represents tension, and a negative N represents compression.

We are considering the beam in its deformed condition. The outer forces are acting, the fibers are subjected to a strain; the axis of the beam has changed its position. At the outset, then, we are unacquainted with the form of the beam we are considering. We know its form when unaffected by forces, but are ignorant of the deformation it undergoes. We are unable to fix the plane of osculation at any point, or the co-ordinate axes x, y, and z, hence we cannot calculate P_x, M_y, M_z, nor find the state of stress in the beam until we have first learned the exact nature of the deformation which those very forces and moments produce. But in order to determine the deformation, we must first learn the state of stress on each part of the beam, and then, combining this knowledge with certain assumptions regarding the

nature of our materials, we can arrive at some conclusions regarding the deformation. We see, then, very clearly here the difficulty — one difficulty—of the problem of finding the exact state of stress and strain. The stress and the strain are functions of each other. In order to find either, some assumption respecting the other must be made. For example, if we assume the deformation we can determine the outer forces and moments, and by discussing the equations of equilibrium written down above, we can find the stresses, and from these the strains, or the movements which the particles have gone through in arriving at the deformed condition; and by considering these movements to be gone through with in the reversed direction, we can see whether the beam would be brought back to its original known shape in a state of repose. If this is not the case, our assumption of the deformation was incorrect. There is, however, another solution of the problem, founded on the fact that in practice the deformations are always very small compared with the dimensions of the beam. We assume, namely, that the deformation is *zero*, determining the outer forces, and from them the inner forces or stresses, under the supposition that the beam retains its original shape, which is supposed to be exactly known. This is the supposition generally made in treating of this subject, and we wished to call attention to its inaccuracy, although its results are practically as correct as we need them. In the rest of this paper we shall consider the beam in its deformed condition, supposed known. In practice the above supposition enables us to compute very close approximations to the value of P_x—M_y, etc., which enter into our equations.

Let ds be the distance between two sections, measured along the axis. ds is supposed infinitesimally small. Since the two sections are normal to the axis, they are parallel to the axis of y, and the distance between them at all points at a distance z from the y axis is

$$ds_z = ds + z d\varphi, \quad . \quad . \quad . \quad (7)$$

$d\varphi$ being the angle between the sections $= \frac{ds}{r}$.

The intensity of the normal stress at any point of the section equals the intensity of strain multiplied by the modulus of elasticity, E. Assuming, then, that the strain on any fiber is expressed by an equation of the first degree, or, S being the strain, and a', b', c', constants,

$$S = (a' + b'z + c'y)ds \quad . \quad . \quad . \quad (8)$$

we shall have for the intensity of the strain,

$$\frac{S}{ds_z} = \frac{(a' + b'z + c'y)ds}{ds + z d\varphi} \quad . \quad . \quad . \quad (9)$$

and for N the value

$$N = E\frac{a' + b'z + c'y}{ds + z d\varphi}\overline{ds} = \frac{a + bz + cy}{ds + z d\varphi}.ds \ldots (10)$$

a, b, c, being new constants.

If the sections were plane before the deformation, the distance between them at any point must have been expressed by an equation of the first degree, or ds' being the distance,

$$ds' = ds(k + lz + my) \quad . \quad . \quad . \quad (11)$$

Hence the strain is

$$ds_z - ds' = ds(1-k) + z(d\varphi - l) - my. \quad (12)$$

and as this is an equation of the first degree ($d\varphi$ being a constant for all points in the same section) this case is a special case of the more general one assumed above.

Substituting in equation (10) $\frac{ds}{r}$ for $d\varphi$, r being the radius of curvature of the axis in o, we have

$$N = \frac{a + bz + cy}{r + z} r \quad . \quad . \quad . \quad (13)$$

Substituting this value in the equations (1), (5), (6), we have the three conditions of equilibrium.

$$P_x = ar\int \frac{df}{r+z} + br\int \frac{z df}{r+z} + cr \int \frac{y df}{r+z} \quad . \quad . \quad . \quad (14)$$

$$M_y = ar\int \frac{z df}{r+z} + br\int \frac{z^2 df}{r+z} + cr \int \frac{yz df}{r+z} \quad . \quad . \quad . \quad (15)$$

$$M_z = ar\int \frac{y df}{r+z} + br\int \frac{zy df}{r+z} + cr \int \frac{y^2 df}{r+z} \quad . \quad . \quad . \quad (16)$$

the integration being in each case extended over the whole section. Now we have the following equations:

$$\frac{r}{r+z}=1-\frac{z}{r+z}=1-\frac{z}{r}+\frac{z^2}{r(r+z)} \quad . \quad . \quad . \quad (17)$$

$$\frac{rz}{r+z}=2-\frac{z^2}{r+z} \quad . \quad . \quad . \quad (18)$$

$$\frac{ry}{r+z}=y-\frac{yz}{r+z} \quad . \quad . \quad . \quad (19)$$

Substituting these values in equations (14), (15) and (16), and putting for abbreviation

$$\int\frac{z^2rdf}{r+z}=\mathrm{I}_0;\ \int\frac{ry^2df}{r+z}=\mathrm{I}_0';$$

$$\int\frac{rzydf}{r+z}=\mathrm{H}_0,$$

the equations (14), (15) and (15) become

$$\mathrm{P}_x=a\int df-\frac{a}{r}\int zdf+\frac{a}{r}\int\frac{z^2df}{r+z}+b\int zdf-b\int\frac{z^2df}{r+z}+c\int ydf-c\int\frac{yzdf}{r+z} \quad . \quad . \quad . \quad (20)$$

$$\mathrm{M}_y=a\int zdf-a\int\frac{z^2df}{r+z}+br\int\frac{z^2df}{r+z}+cr\int\frac{yzdf}{r+z} \quad . \quad . \quad . \quad (21)$$

$$\mathrm{M}_z=a\int ydf-a\int\frac{yzdf}{r+z}+br\int\frac{yzdf}{r+z}+cr\int\frac{y^2df}{r+z} \quad . \quad . \quad . \quad (22)$$

or, putting $\int df=\mathrm{F}$, and observing that since the axes of y and z pass through the center of gravity 0 of the section, we have $\int zdf=o:\int yaf=o$, we have finally,

$$\mathrm{P}_x=a\mathrm{F}+\frac{a\mathrm{I}_0}{r^2}-\frac{b\mathrm{I}_0}{r}-\frac{c\mathrm{H}_0}{r} \quad . \quad . \quad . \quad (23)$$

$$\mathrm{M}_y=-\frac{a\mathrm{I}_0}{r}+b\mathrm{I}_0+c\mathrm{H}_0 \quad . \quad . \quad . \quad (24)$$

$$\mathrm{M}_z=-\frac{a\mathrm{H}_0}{r}+b\mathrm{H}_0+c\mathrm{I}_0' \quad . \quad . \quad . \quad (25)$$

From these equations we find the following values of the constants a, b, c:

$$a=\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_y}{\mathrm{F}r} \quad . \quad . \quad . \quad (26)$$

$$b=\frac{\mathrm{M}_y\mathrm{I}_0'-\mathrm{M}_z\mathrm{H}_0}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}+\frac{\mathrm{P}_x}{\mathrm{F}r}+\frac{\mathrm{M}_y}{\mathrm{F}r^2} \quad . \quad . \quad . \quad (27)$$

$$c=\frac{\mathrm{M}_z\mathrm{I}_0-\mathrm{M}_y\mathrm{H}_0}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2} \quad . \quad . \quad . \quad (28)$$

Substituting these values in equation (13) for it, we find

$$\mathrm{N}=\frac{r}{r+z}\left\{\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_y}{\mathrm{F}r}+\frac{\mathrm{M}_y\mathrm{I}_0'z}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}-\frac{\mathrm{M}_z\mathrm{H}_0z}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}+\frac{\mathrm{P}_xz}{\mathrm{F}r}+\frac{\mathrm{M}_yz}{\mathrm{F}r^2}+\frac{\mathrm{M}_z\mathrm{I}_0y}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}-\frac{\mathrm{M}_y\mathrm{H}_0y}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}\right\}$$

$$=\frac{r}{r+z}\left\{\frac{\mathrm{P}_x}{\mathrm{F}}\cdot\frac{r+z}{r}+\frac{\mathrm{M}_y}{\mathrm{F}r}\cdot\frac{r+z}{r}+\frac{\mathrm{M}_y(\mathrm{I}_0'z-\mathrm{H}_0y)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}+\frac{\mathrm{M}_z(\mathrm{I}_0y-\mathrm{H}_0z)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}\right\}$$

$$=\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_y}{\mathrm{F}r}+\frac{\mathrm{M}_y(\mathrm{I}_0'z-\mathrm{H}_0y)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}\cdot\frac{r}{r+z}+\frac{\mathrm{M}_z(\mathrm{I}_0y-\mathrm{H}_0z)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}\cdot\frac{r}{r+z} \quad . \quad . \quad . \quad (29)$$

This is the most general value of the normal stress. We distinguish the following particular cases:

1°. $r=$infinity. Here $\frac{r}{r+z}=1$, hence

$$\mathrm{N}=\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_y(\mathrm{I}_0'z-\mathrm{H}_0y)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2}+\frac{\mathrm{M}_z(\mathrm{I}_0y-\mathrm{H}_0z)}{\mathrm{I}_0\mathrm{I}_0'-\mathrm{H}_0^2} \quad . \quad . \quad . \quad (30)$$

which is the general formula for a straight beam.

2°. $\mathrm{H}_0=o$. Here we have

$$\mathrm{N}=\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_y}{\mathrm{F}r}+\frac{\mathrm{M}_yz}{\mathrm{I}_0}\frac{r}{r+z}+\frac{\mathrm{M}_zy}{\mathrm{I}_0'}\frac{r}{r+z} \quad . \quad . \quad . \quad (31)$$

This is the formula for the case when the section is symmetrical about the axis of z.

3°. $r=\infty$; $\mathrm{H}=o$:

$$\mathrm{N}=\frac{\mathrm{P}_x}{\mathrm{F}}+\frac{\mathrm{M}_yz}{\mathrm{I}_0}+\frac{\mathrm{M}_zy}{\mathrm{I}_0'} \quad . \quad . \quad . \quad (32)$$

This is the formula for straight beams one of whose principal axes lies in the osculatory plane.

4°. $r=\infty$; $H=o$; $P_x=o$; $M_z=o$:

$$N=\frac{M_y z}{I_o}, \text{ the well-known formula} \ldots (33)$$

The general formula (29) has, so far as the author knows, never before been published. The method used in deducing it, however, is identically the same as the one believed to have been first used by Dr. Winkler, who in his "Lehre von der Elasticität und Festigkeit" (Prag. 1867, page 50) deduced by its means the formula for straight beams

$$N=\frac{P}{F}+\frac{M(I_o'z-H_o y)}{I_o I_o'-H_o^2} \quad . \quad . \quad . \quad (34)$$

In his treatment of curved beams, however, he does not deduce eq. (29), but gets the formula

$$N=\frac{P_x}{F}+\frac{M_y}{Fr}+\frac{M_y z r}{I_o(r+z)}, \text{ cor. to eq.} \ldots (31)$$

The above demonstration has, in common with all others on this subject, assumed the truth of Hooke's law. It is well known that for stresses near the breaking point this law is not true. Prof. Winkler, in his above mentioned book, page 74, has assumed that the normal stress, instead of varying directly as the strain, may be expressed by two terms, one containing the first power, and the other the third power of the strain. He arrives in this way at expressions for the coefficient of rupture for sections of any form.

We have assumed that the strain varies directly as the distance from a straight line in the plane of the section. To find the intensity of the strain, we divide the strain on any fiber by the length of that fiber. We have also assumed that the intensity of the stress varies as the intensity of the strain, but in consequence of the length of the various fibers between the sections being variable, the stress does not in general vary directly as the distance from any line. In fact, equation (29) shows that N is not linear with regard to the co-ordinates. In case 1°, where the radius is infinity, equation (30) shows that N is linear with regard to the co-ordinates; hence the stress varies, directly as the distance from a line of no stress, the neutral axis. To find the equation to the line of no stress in the general case, put for N zero in equation (29), and we obtain as the equation of the neutral axis:

$$o=\frac{P_x}{F}+\frac{M_y}{Fr}+\frac{M_y(I_o'z-H_o y)}{I^2 I_o'-H_o^2}\cdot\frac{z}{r+z}+\frac{M_z(I_o y-H_o^2)}{I_o I_o'-H_o^2}\cdot\frac{r}{r+z} \quad . \quad . \quad (35)$$

As this equation is linear—as is seen by multiplying through by $(r+z)$—we see that the neutral axis is always a straight line. This must, of course, be so, because the assumption in equation (8) shows that the strain is zero along a straight line, and where the strain is zero the stress must be zero. In fact, equation (35) is identical with the equation, $a+bz+cy=o$, as will be easily found by substituting the values of the constants. As equation (35) is not satisfied by the values, $y=o$: $z=o$, it follows that the neutral axis does not pass through the center of gravity of the sections, even when $P_x=o$. But if P_x and M_y are both zero, or if $P_x=o$, and $r=\infty$, then the neutral axis will pass through O. By transposing equations (35) it may be written

$$y[M_y H_o-M_z I_o]Fr^2=z[(P_x r+M_y)(I_o I_o'-H_o^2)+(M_y I_o'-M_z H_o)Fr^2]+(P_x r+M_y)(I_o I_o'-H_o^2)r \ldots (36)$$

hence the tangent of the angle which the neutral axis makes with the axis of y is

$$\tan.\theta=\frac{(M_y H_o-M_z I_o)Fr^2}{(P_x r-M_y)(I_o I_o'-H_o^2)+(M_y I_o'-M_z H_o)Fr^2} \quad . \quad . \quad . \quad (37)$$

This angle will be zero, or the neutral axis will be parallel to the axis of y when

$$M_y H_o-M_z I_o=o \quad . \quad . \quad . \quad (38)$$

We have seen that the equation for N is not linear. An investigation of the properties of this equation would be without practical importance, but we may state that in the form which it takes when $H_o=o$ and $M_z=o$, namely,

$$N=\frac{P_x}{F}+\frac{M_y}{Fr}+\frac{M_y z r}{I_o(r+z)} \quad . \quad . \quad . \quad (39)$$

which represents the most common case

in practice, N does not vary with y, and that if N be plotted at right angles to the section along the Z axis, the curve obtained will be a hyperbola, one of whose asymptote passes through the center of curvature of the axis in O, and is parallel to the axis of X.

With reference to the deformations, we wish to make one more remark which is of interest: Let us take the case of a beam whose axis lies in a plane, in which also the outer forces act—the most common case in practice. M_o is here zero; and the conditions that the neutral axis shall be parallel to the axis of y becomes simply, $H_o = o$. In this case, as shown by equation (39), the stress will be constant along all lines parallel to y, and each section, during the deformation, rotates about the neutral axis, so that the axis of the beam remains in its original plane. In any other case we cannot assent that this will be the case. The condition $H_o = o$ is the same as

$$\int \frac{zydf}{r+z} = o \quad . \quad . \quad . \quad (40)$$

For straight beams this becomes

$$\int zydf = o \quad . \quad . \quad . \quad (41)$$

It is clear that the last equation will be satisfied when the section is symmetrical with respect to the axis of z or y, for then for each positive value of ydf or zdf there will be an equal negative value, and the sum of all the values over the whole section will be zero, identically. But it is well known that any section, whether symmetrical or not, has two axes at right angles to each other, for which the equation (41) is fulfilled. These axes are called the principal axes, and we have the theorem: *The axis of a straight beam, acted upon by forces in a plane, will only remain in that plane when one of the principal axis of each section lies in that plane.* It is believed that this theorem was first stated by Persy.

The ordinary equation for the normal stress, in straight beams, $N = \frac{M_y z}{I_o}$, only applies, then, *when one principal axis of the section lies in the plane of the outer forces.* It is sometimes erroneously applied in other cases.

MR. LAW'S REPORT ON THE TAY BRIDGE.

From "The Engineer."

Mr. Henry Law, M.I.C.E., was employed by Mr. Rothery, Mr. Barlow, and Col. Yolland to examine the Tay Bridge after the fall of a portion of it, and to prepare a report thereon. This report constituted an important portion of the evidence adduced during the trial. We reproduce it complete.

To the Commissioners for the Tay Bridge Casualty.

Gentlemen:—In obedience to the instructions contained in your communication of the 22nd of January, 1880, I have now the honor to lay before you the following Report, embodying the information which I have been able to obtain upon those matters which have a bearing upon the casualty which occurred to the Tay Bridge, on the night of the 28th of December, 1879.

In accordance with your subsequent instructions, in the present report I have confined my attention exclusively to that portion of the bridge which has fallen; and for the sake of brevity and distinctness, I have omitted all reference to those details and particulars of the structure, which, although they may have an important bearing upon the question of reconstruction, have no connection with the cause of the catastrophe.

The bridge, as constructed, consisted of 85 spans, namely, 28 still standing upon the southern side of the river, varying in span from 67 ft. to 145 ft., 13 spans which have fallen, and 44 still standing on the northern side of the river, and varying in span from 162 ft. 10 in. to 28 ft. 11 in.

It will not be necessary to refer to the construction of any other portion of the standing parts of the bridge beyond the two spans immediately contiguous to those which have fallen.

These consist of wrought iron lattice girders resting upon piers, each of which

is composed of six cast iron columns, braced with wrought iron struts and ties, resting upon foundation piers of masonry, brickwork and concrete. The southern span is 145 ft., and the northern span is 162 ft. 10 in. Each girder is 16 ft. 6 in. in height, and their distance apart, from center to center, varies from 9 ft. at their in-shore ends to 14 ft. 10 in. at the ends adjacent to the fallen spans.

These girders rest upon seven cast iron rollers, bearing upon raised surfaces on thick cast iron bearing plates, the rollers having beveled flanges to serve as guides, but there being no attachment between the girders and the piers. The ends of these girders are strengthened to enable them to carry the ends of the larger girders which have fallen, forming a table or shelf upon which the latter girders rested, three cast iron rollers being interposed to allow the girders to expand or contract. These rollers were provided with flanges similar to those below, but there was no attachment between the upper and lower girders. The upright ends of the lower girders were steadied by two transverse wrought iron girders, one at the top and the other at the bottom, with diagonal tee iron stays.

In the portion of the bridge yet standing, the rails are carried upon traverse timber beams laid upon the upper surface of the girders, but in the portion which has fallen the rails were carried upon traverse wrought iron beams, resting upon and secured to the lower booms of the girders.

The length of the portion of the bridge which has fallen is 3,149 ft., consisting of three separate girders, the southernmost one being 1,225 ft. in length, divided into five equal spans, each of 245 ft., the middle girder being 944 ft. in length, divided into four spans, of which the two outer ones are each 227 ft., and the two inner ones each 245 ft., and the northernmost girder, which is divided into four equal spans, each of 245 ft. It will thus be seen that the fallen portion of the bridge consisted of 11 spans, each of 245 ft., and two spans, each of 227 ft.

The gradient of the railway over the southern standing portion of the bridge is a rising one of 1 in 35.368, and this gradient was continued over the first span. Over the second span the gradient changed to 1 in 490, still rising; the line then continued level for six spans, this being the most elevated portion of the bridge; the next span had a falling gradient of 1 in 130, and the remaining four spans had a falling gradient of 1 in 73.56, which continues over nearly the whole of the northern portion of the bridge.

The course of the railway over the fallen portion of the bridge, and for a considerable distance upon each side of it, was a continuous straight line.

The fallen portion of the bridge consisted of wrought iron lattice girders 27 ft. in height, placed at a distance of 14 ft. 10 in. apart from center to center. The upper and lower booms were trough shaped, being each 2 ft. in width, and between 15 in. and 16 in. in depth. The girder over each span was complete within itself, the vertical ends being of similar section to the booms, only 18 in. in width upon the face; the lattice bars, which had only a tensile strain to resist, consisted of flat bars in pairs, one being riveted to each side of the booms; those which were in compression consisted of I-shaped struts placed between the sides of the booms, and secured to them and to the tensile bars at their intersections.

The upper booms were braced by transverse wrought iron beams with diagonal stays. The railway was carried upon transverse wrought iron fish-bellied girders about 5 ft. 5 in. apart, which rested upon the upper side of the lower booms, and being riveted thereto served as struts to the girders, the bracing being rendered complete by diagonal angle iron stays, crossing through the center of each alternate transverse girder. In order to lessen the transverse strain upon the bottom boom, suspension bars of wrought iron were attached to the lattice bars at their intersections, and riveted at their lower extremities to the sides of the boom.

The various parts of these girders have been carefully proportioned to the several strains to which they had to be exposed, and as the catastrophe did not result from the failure of these girders, it is not necessary more particularly to describe them. It is, however, desirable to make an observation with reference to how far each division should be regarded as having formed a continuous girder.

As already mentioned, each girder was complete in itself, and the booms of these separate girders were connected by cover plates with the intention of making them continuous; but in the face of the evidence given at Dundee, of the manner in which these connections between the girders were made, I do not think that these divisions can be considered to have been continuous in such a manner as to produce an increased pressure upon any of the piers. It was stated by William Oram—Question 6494—that the connecting cover plates were temporarily secured by service-bolts, which were afterwards removed and replaced by rivets; the bridge in the meantime being used for the passage of heavy ballast trains—Questions 6821 to 6825. It is true that the ends of the girders had been originally raised before the cover plates were bolted on; but it must be evident that no strain such as would produce continuity in the girders in the sense now under consideration could have existed, for if it had it would have been quite impossible to have removed any of the bolts.

Judging from the portion of the bridge which is standing, the permanent way appears to have been very carefully constructed. The rails are laid upon longitudinal timbers, or way-beams, 18 inches wide by 15 inches in depth, the rails themselves are of steel, 75 lbs. to the yard, with guard rails of the same weight and material, both rails being secured in the same chairs, which are placed 3 feet apart; a flat wrought iron tie bar is also introduced at distances of about 19 feet apart to preserve the line in gauge.

The platform of the bridge was formed of planks 4 inches in thickness, covered with asphalte and with a few inches of ballast as a preservative against fire.

I now proceed to describe the most important part of the structure in connection with the subject under consideration, namely, the piers upon which the fallen portion of the bridge was supported.

These piers each consisted of an assemblage of six cast iron columns, braced by means of wrought iron studs and ties. Their foundations consisted of hexagonal-shaped piers of concrete, faced with brickwork, measuring 27 feet 6 inches in length from point to point of the cutwaters, and 15 feet 6 inches in width. These piers were carried to a height of 5 feet above the level of high-water of spring tides, the upper four courses being faced with stone, and no movement or settlement appears to have taken place in them.

The height from the top of the upper course of the masonry to the under side of the lattice girders varies from 83 feet to 81 feet 3 inches; in the following description, and in all the calculations the highest pier is referred to; as, however, the height of the pier affected the strength, it may be desirable to give in a tabular form the heights of the several piers above the masonry and the spans of the girders which they supported; the numbers in the first columns are the numbers of the piers in the structure, counting from the southern side, and, to avoid confusion, will be adhered to throughout this report.

No. of pier.	Height of pier.		No. of span.	Width of span.	Description of bearing on pier.
	ft.	in.			
28	67	6	29	245	3 rollers on lower girders.
29	82	6	30	245	8 rollers on pier.
30	83	0	31	245	8 rollers on pier.
31	83	0	32	245	Bolted to top of pier.
32	83	0	33	245	8 rollers on pier.
33	83	0	34	227	6 rollers and an expansion joint.
34	83	0	35	245	8 rollers on pier.
35	83	0	36	245	Bolted to top of pier.
36	83	0	37	227	8 rollers on pier.
37	82	8	38	245	6 rollers and an expansion joint.
38	82	4	39	245	8 rollers on pier.
39	82	0	40	245	Bolted to pier.
40	81	8	41	245	8 rollers on pier.
41	66	10	—	—	3 rollers on lower girders.

Cast iron base pieces, 2 feet in height, for the reception of the columns, were secured to the piers, each piece having four holding-down bolts passing through the upper two courses of masonry, each of which was 15 inches in thickness.

The six columns were arranged so as to form two clusters, each triangular on plan, and having no other connection at their upper extremities beyond the struts and ties. The two extreme columns, 1 and 4, were each 18 inches in diameter, and inclined inwards at the top 12 inches in their whole height; the other four columns, 2, 6 and 3, 5, were each 15

inches in diameter. They stood in vertical planes parallel to the direction of the bridge, but in those planes 2 and 6 and 3 and 5 were each inclined 12 inches towards each other in their whole height.

Each column was composed of six flanged pipes, connected at their joints with eight screwed bolts, each $1\frac{1}{8}$ inch in diameter. Each triangular cluster was surmounted by a wrought iron box girder L-shaped on plan, taking its bearings upon the three columns; and upon the box girder another wrought iron cellular girder was placed, running in the direction of the axis of the bridge, and vertically under the longitudinal lattice girder of the bridge itself. Upon the upper side of this cellular girder was bolted a massive cast iron plate, a similar plate being also bolted to the underside of the longitudinal lattice girders of the bridge, and between these two plates were placed the cast iron rollers, each 15 inches in diameter and 2 feet in length, upon which the weight of the bridge was carried. This description applies to all the piers, excepting Nos. 31, 35 and 39, in the case of which piers the rollers were omitted, and the longitudinal lattice girders were united to the cellular girders by screwed bolts.

Measuring across the bridge, the cellular girders were equally distant from the centers of the tops of columns 1, 2 and 6, and 3, 4 and 5, and consequently the pressure of the girders of the bridge was borne half by each outer 18-inch column, and one-fourth by each inner 15-inch column.

The three columns forming each triangular group were braced to each other at every joint by wrought iron struts and ties: the struts were horizontal and consisted of two channel irons placed back to back and bolted at each end by two $1\frac{1}{4}$-inch bolts to lugs cast upon the columns. Each of the rectangular openings formed by the columns and struts was stayed diagonally by flat wrought iron bars $4\frac{1}{2}$ inches broad and $\frac{1}{2}$ inch in thickness, the upper ends being connected with the columns by $1\frac{1}{8}$-inch bolts passing through lugs cast upon them, and the lower ends being secured to two sling plates, each $4\frac{1}{2}$ inches by $\frac{3}{8}$ inch thick, by gibs and cotters, and the sling plates being connected with the columns by $1\frac{1}{8}$-inch bolts passing through lugs cast on to them.

The two triangular clusters of columns were braced to each other in a similar manner by struts and ties between the 15-inch columns; that is to say, between columns 2 and 3, and columns 5 and 6. Furthermore, at each joint a wrought iron rod $1\frac{1}{2}$ inch in diameter was introduced horizontally to tie together columns 2 and 5, and columns 3 and 6.

Having thus given a general description of the portion of the bridge which fell, I proceed to consider the strains to which the several parts were exposed under varying circumstances, and how far the structure was capable of resisting these strains. In order, however, to render this Report as brief as possible, and to avoid as far as can be done the introduction of technicalities, I shall here confine myself to a statement of results, but for your information the mode of arriving at these results is annexed in the form of an appendix.

The four forces to which the structure was liable to be exposed were those resulting from changes of temperature, from the weight of the structure itself, from the weight of a passing train, and from the lateral pressure of the wind.

For our present inquiry the strains produced by changes of temperature may be disregarded, and those resulting from the weight of the structure itself, or when loaded with a train, are very easily ascertained. Assuming for the reasons already stated that no additional strain is produced upon any of the piers in consequence of the continuity of the girders, and assuming a train with the weight and conditions of that which fell with the bridge, namely, having a weight, including the passengers, of 120 tons, and supposing it to be placed over one of the piers in the position which would produce the heaviest pressure, I find that the structure alone would produce a compressive strain upon the 18 inch columns of 1.47 tons, and upon the 15-inch columns of 1.06 tons to the square inch; and that with the train over the pier these strains would be increased to 1.84 tons on the 18-inch columns, and to 1.30 tons on the 15-inch columns.

There are so many doubtful elements, the value of which have to be assumed in attempting to determine the amount

of the strains to which the several parts of the piers would be exposed by the action of a powerful wind pressure, that it is impossible to arrive at any positively definite result.

As regards the actual pressure of the wind upon the structure, I have adopted the same views as those taken by Dr. Pole and Mr. Stewart, namely, as regards the lattice girders; for the windward girder I have taken the entire area of the outer face, including the way-beams and rails; for the leeward girder I have taken only the surface above the level of the rails, and I have supposed that the wind would only exercise half its force against this surface, in consequence of the shelter afforded by the windward girder. As regards the train, I have wholly deducted the surface of the leeward girder which it would shelter, and for the train itself I have only taken half the round surfaces, and have reduced the pressure of the winds by a sixth, that being the extent to which the train would be sheltered by the windward girder.

In the case of the pier I have again adopted the views of Dr. Pole and Mr. Stewart, namely, in supposing that there would be one 18-inch column and three 15-inch colums exposed to the wind, and that the tie-bars and struts would be equivalent to one-fourth of the space—when seen in end elevation—between the columns.

Now, it is a matter of the first importance to determine what wind pressure would suffice to overturn any portion of the train; it is at once evident that the second-class carriage, being the last but one in the train, was the one which had the least stability; and Dr. Pole and Mr. Stewart state that a wind pressure equal to 28½ lbs. upon the square foot would suffice to overturn this carriage. They have, however, assumed that the carriage was empty, whereas the evidence of those who collected the tickets shows that there were eight second-class passengers.

In my own calculation I have assumed the average weight of the passengers at 140 lbs. each, and I have taken into account the vertical pressure resulting from the action of the wind upon the curved surface of the roof, and the conclusion at which I arrive is that the second-class carriage could not have been overturned with a less wind pressure than 35.68 lbs. upon each square foot; and as there is no position in which this carriage could have been placed where it would have been sheltered to a greater extent than between one-seventh and one-eighth of its entire surface, it results that the actual pressure of the wind must have exceeded 40 lbs. on the square foot to have overturned this particular carriage, in the condition in which it was upon the night of the catastrophe, and without regarding any assistance which the couplings might afford in retaining the carriage upon the rails.

The next subject that I have investigated is the effect which the wind would have in lessening the weight of the superstructure upon the windward rollers, and in increasing the same upon the leeward ones, and the results are shown in the following table:

	Without any wind.	With pressure of wind equal to			
		10 lbs.	20 lbs.	30 lbs.	40 lbs.
	lbs.	lbs.	lbs.	lbs.	lbs.
Without any train:					
Pressure on west rollers ..	322,450	300,120	277,930	255,670	233,410
Pressure on east rollers...	322,450	344,710	366,970	389,230	411,490
	644,900	644,900	644,900	644,900	644,900
With a train:					
Pressure on west rollers ..	427,615	399,205	370,795	342,385	313,975
Pressure on east rollers...	427,615	456,695	485,775	514,855	543,935
	955,230	855,900	856,570	857,240	857,910

The slight increase which will be observed in the total pressure upon both rollers with an increased wind is owing to the vertical pressure resulting from the action of the wind on the curved roofs of the carriages.

These pressures upon each set of rollers are, as I have already explained, equally divided between one 18-inch and two 15-inch columns; these pressures are, however, still further modified by the horizontal pressure of the wind acting against the exposed surfaces of the superstructure, pier and train, but to what extent it is very difficult to determine.

If for a moment it is assumed that the pier may, by virtue of the system of bracing, be considered as a rigid structure, and the effect of the bolts in holding down the columns be disregarded, then the wind pressure required to overturn the structure, about the east 18-inch column as a center, would be 36.38 lbs. without any train, and 32.69 lbs. on the square foot, with a train over the pier.

But, unfortunately, the piers must have been very far from being rigid structures, in consequence of the imperfect manner in which the struts and ties were connected with the columns. The struts consisted of channel irons, placed back to back with the lug of the column between, and connected therewith by two $1\frac{1}{8}$ inch bolts at each end; the holes for the bolts were cast $1\frac{1}{4}$ inch in diameter, and being rough and larger than the bolts, and the ends of the struts having no bearing surface to abut against, the struts themselves were only retained in their positions by the pinching action of the bolts. But the security thus afforded must have been very slight, because, owing to inequalities in the surfaces of the lugs themselves, and to the fact that in some cases the holes in the struts had been roughly enlarged with a blunt tool so as to leave a burr, the actual bearing surface of many of the struts against the lugs was very small.

As regards the flat ties, when the structure was first erected they were tightened up by means of gibs and cotters, but, owing to the slots in the bars against which the gibs and cotters bore being rough, and the gibs and cotters also roughly forged, and, further, owing to the holes cast in the lugs not being cylindrical, and to a screwed bolt being used to secure the ends of the ties instead of a pin, the real bearing surface was exceedingly small, and a comparatively slight strain would suffice to crush the edge of the hole in the lug into the thread of the screw.

In reference to the tie bars it should also be observed that the bearing surface of the gib against the slot in the bar was quite inadequate, for while the area of the section of the bar exposed to a tensile strain was 1.625 square inches, the bearing surface of the gib being in compression should have had an area of 1.86 square inches, whereas it had only a surface of 0.375 square inches, or about one-fifth of the strength of the bar.

From these circumstances it would result that a lateral pressure against the columns would produce movement in the struts and ties, resulting in the latter becoming slack. And this movement actually did take place; in some of the tie bars still standing I found packing pieces of iron $\frac{1}{4}$ inch in thickness had been introduced between the gibs and cotters, and on inquiry I learned that these had been introduced from time to time since the opening of the bridge.

From the accounts which have been furnished to me it appears that about 150 of these packing pieces were inserted in the ties between the middle of October, 1878, and the time of the bridge falling, and that the necessity for them arose before the bridge had been opened five months. This circumstance clearly shows that there must have been a considerable racking movement in the piers under the united action of passing trains and wind, and I cannot but consider points to the primary cause of the disaster.

For the slackening of these ties and struts means the removal of that condition upon which alone the power of the structure to resist being overthrown by a lateral pressure depends. And it is easy to conceive that a storm of the violent character of that of the 28th of last December, would produce such movements, in the connections of these struts and ties with the columns, as would render the columns unable to sustain the additional weight of the train and the lateral pressure of the wind.

An examination of the ruins of pier

No. 32, being that over which the train was situated when the structure fell, indicates that the columns doubled up about their joints as the lower lengths of the westward 15 inch columns were pushed over to the west, or in the reverse direction to that in which the rest of the structure fell. A similar action in pushing back the westward columns is seen in piers Nos. 36, 39, and 40.

The present state of piers Nos. 29 and 31 affords conclusive proof of a weak point existing in the structure at the time of the overthrow in each of those piers, namely, in pier No. 29 at the level of the top of the second tier of columns, and in pier No. 31 at the top of the lower tier; for the strain at the point of fracture was, in the former case, only five-sevenths, and in the latter case only six-sevenths, of the strain at the base of the pier, while theoretically the strengths of the pier at the base and at the points of fracture were the same. It is clear, therefore, that the power of resistance of these two piers had been reduced at the points of fracture in the case of pier No. 29 to the extent of two-sevenths, and in the case of pier No. 31 to the extent of one-seventh of their normal power of resistance.

Considering that the columns are 76 ft. in height, that with a wind pressure of only 20 lbs. on the square foot, a pressure of 337 tons will be thrown upon the eastward 18 inch column at the time of the passage of the train, and that a horizontal pressure of 37½ tons is acting against the top of the column, it is easy to conceive what must have been the inevitable consequences of any slackness on the part of the ties.

It is also necessary to point out that owing to the double angle which the ties, by which the 18 inch columns are braced, make with the direction of the force tending to overthrow the structure, the efficiency of these ties is reduced in the proportion of 1 to 2.73, or to little more than one-third of their full strength, and that any elongation or movement of the ties would allow of nearly three times that movement in a horizontal direction in the point of the column to which they were attached.

There are also other circumstances in connection with the construction and workmanship of the bridge which undoubtedly contribute to the catastrophe.

The mode of securing the holding-down bolts was not satisfactory, as they had no anchor-plate or bearing at their lower extremities, but were merely inserted in a hole drilled through the two 15 inch courses of stone, and were then run round with cement, and, as the angle of taper of the conical head was only 6¼ degrees, it is evident that a very slight compression of the cement would allow of a considerable movement in the bolt; some of these bolts have evidently yielded as much as 8 inches in screwing down the base-piece at the erection of the bridge, and in one or two cases the stones have been burst by the wedge action of the conical head. It would have been better also if they had been carried to a greater depth, so as to have had a greater weight of masonry to be lifted instead of trusting to the adhesion of the cement, which appears to have been very slight, partly in consequence of the smoothness of the sawn face of the stone, and partly, I imagine, from the stone having been dry when set. In many cases the cement has parted from both stones, forming a thin detached sheet of large dimensions. In many cases also the nuts at the upper ends of the bolts have a very imperfect bearing upon the base-piece.

Passing on to the columns, it is apparent that many of them have blow-holes of considerable size, which have been filled in with a composition of resin and filings; sufficient care does not appear to have been taken to keep the cores from shifting, or in properly adjusting the upper flask, and as a consequence there are many instances of a considerable difference in the thickness of metal on opposite sides of the columns; in some cases the metal on one side being only ⅜ inch, and on the opposite side 1⅛ inch, or a difference of ¾ inch; and as is usually the case when the upper side of a casting is thin, the metal becomes chilled, and has accumulations of scum and air which very much deteriorate from the strength of the metal.

The mode of attaching the ties to the columns by means of lugs was evidently insufficient, as in almost every instance the lugs have been torn away, it

is difficult to believe that the burning on of defective lugs in the manner described by the witnesses examined at Dundee could have been sanctioned by any person who had the intelligence to understand that the whole security of the structure depended upon the strength of these lugs.

I consider that the mode of connecting the columns at the flange joints was also in some respects defective, the bolts being $\frac{1}{8}$ inch less in diameter than the hole, and the flanges being separated in some cases as much as $\frac{3}{4}$ inch, the bolts could not act as steady-pins, and as in several cases there was no spigot on either of the pipes, there was nothing but the pinching of the bolts to prevent the columns from shifting, and there are evidences that some of them did so shift at the time of the catastrophe.

I have not regarded the concrete as having added in any way to the security of the structure, otherwise than in its increasing the weight of the columns, and so increasing the moment of stability of the pier; and my reason for taking this view is that the concrete was so unequal in its quality that no dependence could be placed upon its being of proper strength in the place where strength was required.

Before leaving the columns, I should observe that some of the flanges were so imperfectly faced that the only portions of the metal in contact was a strip of about $\frac{5}{8}$ inch round the margin of the flange.

In conclusion, I would sum up by the statement that, in my opinion, the base of the pier was too narrow, occasioning a very great strain upon the struts and ties, that the angles at which the latter were disposed, and the mode of connecting them to the columns, were such as to render them of little or no use, and that the other imperfections which have been pointed out lessened the power of the columns to resist a crushing strain; I consider that the yielding of the struts and ties was the immediate cause of the disaster, but that the other circumstances stated contributed to it.

It is only due to Sir Thomas Bouch, to his assistant, Mr. Thomas Peddie, to Mr. Noble and the officials of the North British Railway, to say that they have afforded me every facility for making the most thorough and searching investigation.

ON THE PHYSICAL ASPECTS OF THE VORTEX-ATOM THEORY.

By S. TOLVER PRESTON.

From "Nature."

In all attempts to arrive at a satisfactory conception of the ultimate constitution of matter, the grand difficulty has hitherto been to reconcile the proved indestructibility of the atom with its capacity for executing vibrations, as demonstrated by the spectroscope. The ancients, by assuming the atom to be *infinitely hard*, attempted in this way to get over the difficulty of indestructibility (or indivisibility), but thereby debarred all means of conceiving the "*elasticity*" of atoms, or their known powers of taking up vibrations of different periods.

When we consider the immense difficulty that there must have been in conceiving how an atom could be elastic (*i. e.*, how its parts could be capable of free motion) and yet its parts be incapable of separation from each other, we may well excuse the attempt to explain indestructibility by the assumption of the quality of *infinite hardness*, unsatisfactory though it might be.

It is evident that if we are to renounce all idea of occult qualities of "elasticity," hardness, indivisibility, &c., and purpose to explain the facts without recourse to postulates, we must assume the material substance of which our atoms are to be formed, to be itself entirely without any positive qualities, *i. e.*, to be without elasticity, hardness, rigidity, &c., and therefore to be freely penetrable in all parts, or perfectly passive and inert. This is the perfect liquid of the vortex-atom theory. There may be some who would say that it is difficult to conceive

of such a liquid. On the contrary, we venture to be able to prove that such a liquid *always is conceived of* whenever a liquid is thought of. Thus, does any one in conceiving of a liquid (water, for instance), regard the liquid as consisting of solid (*i. e.*, more or less rigid) portions of matter sliding over each other [as we might conceive solid masses sliding past or through each other on a magnified scale]; and yet this is truly what the liquid (composed of molecules) is in the actual fact. In short, it is not a "*liquid*" at all. Yet we conceive of it as *liquid, i. e.*, freely penetrable in all parts. We therefore contend that a perfect liquid (or true liquid) is what is *always* conceived of, and therefore that there can be no difficulty in regard to the conception of the true liquid that forms the basis of the vortex-atom theory.

In the next place, it is an obvious condition to any consistent conception of matter that matter must possess *extension*,* or occupy space, *i. e.*, so that two portions of our liquid cannot occupy the same space at the same time. If, therefore, the liquid fills all space, it must be incompressible. This is, therefore, not an arbitrary postulate.

The next question naturally suggesting itself would be, how are portions of such a liquid to attain the properties that we recognize in atoms? We venture to think it will be conceded as evident that the only *conceivable* way (if it be admitted that the result is attainable at all) is through *motion* [for this is the only conceivable way in which the liquid can be affected.] The further inquiry would therefore be, what would be the *character* of this motion? Now, in order to fulfill the condition that the atom itself can be brought to rest without losing its properties as an atom, it is evident that the motion of the material forming it must take place in such a way that the atom can remain in one spot, or be to our senses at rest, *i. e.*, the material of the atom, although in motion, must not deviate from one spot. We ask if there is any other *conceivable* form of motion than *rotary* motion that would fulfill this condition? Hence the necessity for looking to *rotary* motion as the basis of the properties of the atom. In the next place, a portion of material in rotation must rotate about an *axis*. If the ends of this axis were exposed, we should have two points *at rest*, which would forfeit the condition of *motion* being the essential basis of the external qualities of our atom. The question is, therefore, how is a portion of material to be in rotation about an axis, and yet not expose the ends of the axis? The only *conceivable* answer (as we think will be admitted) is that the rotating portion of material must have the form of a closed ring, or complete circuit, so that the axis has no ends. We therefore think it may be said beforehand that conceding that the problem of the atom can be solved at all (or if it be conceded that a fact can exist solely in virtue of the explanation that underlies it) then the problem could only *conceivably* be solved under the fundamental conditions above developed, *i. e.*, under the condition of a portion of material (having no positive properties in itself) *rotating* in the form of a *closed circuit*.

This (as is well known) is what has been found to satisfy the conditions for the atom by the application of mathematical analysis (without, apparently, that object having been in view at all), and in a manner the most remarkable in its completeness. It appears possible, in view of the above considerations, that a profound and competent thinker who had devoted himself to the subject might have arrived, even before the mathematical analysis had been applied, at the *sole conceivable* physical conditions that in principle could satisfy the problem of the atom (admitting the *existence* of the solution); but the mathematical analysis can, of course, alone make the fact of the solution apparent to us. It is related in the article on "The Atomic Theory of Lucretius" (*North British Review*, March, 1868) that Hobbes had arrived at the fundamental idea that the *rotation* of a portion of material must be the basis to the solution of the problem of the "elasticity" of the atom, without having applied any mathematics.

The difficulty of the mathematical side of the vortex-atom theory is curiously contrasted with the simplicity of the physical side of the theory. If we suppose a cylindrical bar of india-rubber to be rotated about its longitudinal axis,

*The quality of extension may even be regarded as included in the definition of matter.

and the bar (still rotating) to be bent round into a ring shape and the ends joined (the rotation of the material of the ring being always continued), then this may serve to illustrate in a simple way the motion of the material forming the vortex-atom. It is here apparent that the material of the india-rubber ring (in our illustrative case) may be in rapid motion while the ring itself preserves a fixed position in space. It would seem to be a pity if a spurious mystery should be allowed to envelop this subject, which is unworthy of it, in view of the simplicity of its physical basis. No one doubts the difficulties that had to be surmounted on the mathematical side of the theory, but there is all the more reason on that account that the extreme simplicity of the physical side of the theory should be duly appreciated, and unnecessary obstacles not be thrown in the way of its adoption. The tendency to invest physical subjects with a halo of the occult [possibly partly attributable to the unfortunate introduction into physical science of the spiritualistic conception of "*force*"—in the sense of an action across space without the intervention of matter] has probably done more to hinder progress than any real difficulties.

We shall simply state the facts of the mathematical analysis here, our business being more particularly with the physical side of the theory. First it is shown by incontrovertible mathematical proof that a portion of material having the motion above described possesses all the qualities of a *solid*. It is at the same time "*elastic*," or capable of changes of form when acted on through impact by other atoms —always tending to return to its symmetrical form when removed from constraint. It is, moreover, proved to be competent to execute vibrations of definite periods which it is the function of the spectroscope to measure. The atom thus constituted is demonstrated to be incapable of being divided or severed by the collisions of other similar atoms against it, and *since this is the sole means of acting upon it*, the long-standing riddle of indestructibility is thus simply solved, without the necessity for any postulate of *infinite* hardness. As the degree of hardness merely depends on the velocity of rotation of the material, it follows that the vortex-atom may possess any degree of hardness. Indeed, if we imagine the atom to be magnified up to visible scale, it might be conceived to be harder or more rigid than a ring of steel of the same dimensions, since the hardness of steel is limited by the resistance of the component atoms to displacement.

The centrifugal tendency of the rotating material of the vortex-atom is controlled by the exterior incompressible liquid, and as there is no friction [there being no ultimate solid parts in the rotating liquid to "catch" against the inclosing fluid walls], the rotating portion therefore glides smoothly over the incompressible liquid that surrounds it like a pipe. Indeed, if we leave out of our conceptions the portion of rotating liquid, then the surrounding liquid actually forms a complete pipe in the form of a closed ring. If the liquid in the pipe were to fly out, a temporary void would be formed in it, which is impossible in a liquid that already occupies all space. An idea of the resistance of such a rotating portion of material to bending may be got by attempting to deflect a gyroscope or spinning-top.

In the old idea of *infinitely* hard atoms there were difficulties in forming a satisfactory conception of what took place at the collision of two such atoms, or how the rebound could effect itself (consistently with the conservation of energy). The following difficulty may also be mentioned: Since two such atoms are supposed to be absolutely hard or unyielding, the area of contact at the collision would necessarily be merely a mathematical point. Now the intensity of a given pressure on a surface is inversely as its area; and accordingly, since the area is here a mathematical point (or infinitely small), the pressure attendent on the collision of the two atoms would require to be *infinitely* great. It may be a fair question how even an *infinitely* hard atom is to withstand the disintegrating influence of an *infinite* pressure.*

In the case of the vortex-atoms they

* The fact of two such infinitely hard atoms being stopped in an infinitely short space at collision [for there is by hypothesis no *gradual* yielding] would by itself entail an infinite pressure *in addition* to the infinite pressure due to touching at a mathematical point.

yield somewhat at collision (without change of volume, of course), whereby the encounter takes place over a surface (not a point); and they rebound in virtue of their elasticity, due to the motion of the material forming them.*

There would seem to be a view to a certain extent prevalent that the vortex-atom theory essentially alters the basis of the old-established ideas of solid indestructible atoms surrounded by space in which they can freely move, to which so many have accustomed their conceptions, and worked upon to the successful discovery of new facts, and which ideas, therefore, they might be reluctant to abandon. This step, however, is not required at all. The main purpose of the vortex-atom theory is to explain the "*elasticity*" of atoms, retaining substantially everything else appertaining to the old atomic theories, merely removing the unsatisfactory postulate of *infinite* hardness. For since the perfect liquid (outside the portions of it that form the atoms) opposes no resistance whatever to the passage of the atoms through it, or it is impossible to act on the exterior liquid, it is therefore in this respect as if a void existed outside the atoms. It is desirable, however, to note that the vortex-atom theory involves essentially the *existence* of the liquid outside the atoms, which performs important functions, but since this exterior liquid is proved to be incapable of appealing to our senses in any way, it therefore *in that respect* may be said to play the part of a void. The exterior liquid of the vortex-atom theory corresponds to the void space of the theory of Lucretius. With the above qualification, therefore, it may be allowable, when we are not specially dealing with the problem of the constitution of the atom itself, to leave out of our conceptions the presence of the exterior liquid; that which we call "matter" being the atoms, and not the exterior liquid. In all practical problems of physics, therefore, (apart from the problem of the constitution of the atom), we may properly regard the atoms simply as *elastic* indestructible solids moving freely in space. Moreover, since the motion of rotation of the material of the atom is incapable of transference, and cannot appeal to our senses, and this motion does not in any way alter the position of the atom in space [but it is exactly as if the atom *itself* were at rest]; we can, therefore, if we like, leave this rotary motion out of our conceptions, merely keeping in view the result produced by the rotation, viz., the sharply-defined elastic indestructible solid thereby formed. The function of the modern theory is accordingly not to destroy the atomic theory of the ancients, but rather to support it, by explaining *how* such indestructible bodies can exist, without recourse to the unacceptible postulate of *infinite* hardness. This old theory of the atomic constitution of matter was really too firmly grounded on reason and observation, as that one should suppose that its very foundations could be shaken.

Broadly and generally, therefore, in practical problems of physics, the essential points to recognize are that atoms—or molecules—are elastic indestructible bodies, capable of rebounding from each other without loss of energy, and of executing vibrations of fixed periods. The existence of this *elasticity* is a fact so definitely proved by the spectroscope, which actually measures the *number* of vibrations executed per second by molecules, that it would become a question to *explain* this fact, even if the vortex-atom theory had not been proved to be capable of affording a complete explanation of it. Indeed, not only is the theory *capable* of doing this, but the vibrating capacity possessed by molecules is shown to be a *necessary* consequence of the theory, so that, therefore, the fact might even have been deduced *à priori*. Considering how enormously difficult it appeared to account for this fact at one time, or how impossible it seemed to reconcile the mobility of the parts of a molecule with the inseparability of these parts by the most energetic collisions, and how an explanation of this fact was at one time sought after, it would appear not too much to expect that those who hesitate to accept the explanation given by the vortex-atom theory, should endeavor to define for themselves wherein their grounds of objection lie. For if the explanation of a fact be admitted to

* The rebound of vortex-atoms may be illustrated (as is known) roughly by the rebound of two smoke-rings from each other, or by the rebound of vortex-rings in an ordinary (imperfect) liquid.

be substantially complete, it would be at least unreasonable to look for more. The question might also suggest itself as a fitting one to any impartial inquirer, whether any other solution to the problem of the constitution of the atom is in principle *conceivable*, or whether [as in the case of many other physical problems, the constitution of the ether, for instance] but *one* solution is conceivable (or we have no choice at all). It cannot be said at least that the theory of vortex-atoms, or its physical side, is not *simple*, dealing as it does with the mere *rotation* of a portion of matter.* It is so far recognized that simplicity of the means to the end is a general characteristic of nature. No doubt there may be difficulties in the mathematical development of the subject; but if an atom be once proved to be elastic and indestructible, that fact surely goes very far to supply all we want for the practical applications of the theory. Of course there may be some refinements that may present great mathematical difficulties. For instance, Prof. Tait in his work, "Lectures on some Recent Advances in Physical Science," mentions a case where a vortex-ring is supposed to come into collision with another in such a way that the motion is not symmetrical in relation to the axis, and it is cited as an almost insurmountable difficulty to find what exactly takes place (in regard to particular vibrations or rotations developed, possibly). But one might ask, is it necessary to know this for practical problems of physics? We may know broadly that vibration or rotation is developed, and if so (apart from the abstract interest of the question), do we want to know precise quantitative details for practical purposes? It might for example be extremely difficult to determine mathematically the exact deformation or changes of form (vibrations, &c.) that a steel ring underwent when thrown against the hard surface of an anvil; but the practical question is, do we want to be acquainted with this for any ordinary problem that might occur, or in order to appreciate the general principles of impact, for instance? So in the case of vortex-atoms, no doubt many instances might be cited when it would be difficult to ascertain precise results, but the practical question is, Does this prevent our applying the theory to ordinary physical problems,* or to dynamical phenomena involving questions of principle? For possibly it may not be necessary to know the exact vibrations developed at a collision (for instance), provided we recognise the fundamental point that energy is conserved, and that the atoms can rebound from each other like perfectly elastic solids. It would be a pity if the mere *difficulty* of arriving at precise mathematical results of a refined character, should be mistaken by some for *mystery*, or it would be a thing to be regretted if there should be any tendency to throw a veil of the "occult" over what in its *physical basis* (at least) is very simple, this procedure only hindering progress and rendering a closed book what might be a most interesting branch of mechanics.

The investigations regarding the perfect liquid have already (as is known) thrown some important light on the important practical question of the resistance of ships. Mr. Froude has especially devoted himself to these inquiries.

*It would seem to be thought by some that the primary *ring* form of the vortex-atom involves something complicated in it. I venture to think that this is only one of those first impressions, which will disappear on reflecting on the subject. First, many facts strongly indicate that matter possesses a more or less *open* structure (or is highly porous). These ring molecules would give matter an open structure. It would seem also independently probable that a molecule should have no more material in it than is essential to give it a certain amount of *extension*, or to make it occupy a certain range of space. Why should we suppose that waste or apparent superfluity of material in a molecule that a solid structure throughout would involve? Does not this violate one of the fundamental principles of large scale architecture, where superfluity of material is recognized as one of the worst faults, and mechanical principles are admittedly independent of scale? The *ring* shape for the atom is evidently the simplest elementary form to satisfy the condition for the maximum of *extension* combined with the minimum waste or expenditure of material. In view of these considerations, the ring-shape, the primary form required by the vortex-atom theory, may seem in itself independently probable. Indeed, it seems a remarkable fact that the main conditions inevitably led up to by this theory by a rigid mathematical process, are precisely those that independent observation support, (1) the *indestructibility* of the atom, illustrated by chemistry and numerous facts, (2) the *elasticity* of the atom, proved by the spectroscope, (3) the *open* structure of the atom, in harmony with the transparency of some bodies to light, the free passage of the magnetic disturbance through all bodies, and numerous other facts—not to mention the physical theory of gravity. In short, it would appear that it would be necessary to infer the *existence* of indestructible elastic atoms of open structure, even if the vortex-atom theory (which *explains* this fact) had not been invented.

*The writer himself has seen from German comments on Prof. Tait's work, that the passage above referred to [German translation] has been regarded by some as if the difficulty there mentioned were of such a nature as to prevent the practical adoption of the theory.

The old idea that a ship (or more correctly a totally immersed body, such as a fish) encountered a mysterious resistance in addition to the mere friction of the molecules of water on its sides, is now known to have been a pure delusion. If it were not for the fact that the water consisted of molecules or ultimate rigid parts which are caught and put in motion by the rough sides of the ship, there would be demonstrably no resistance at all. Hence the absence of resistance in a true liquid (which is not formed of ultimate rigid parts or molecules). If the molecules or ultimate rigid parts of which an ordinary "liquid" consists, were to be liquefied, a being immersed in it would (if conscious) imagine he was surrounded by empty space.

The late Prof. Clerk Maxwell in a review of the theory of vortex-atoms in the "Encyclopædia Britannica" for 1875, under the word "*Atom*," makes the following remark on the theory:

"But the greatest recommendation of this theory from a philosophical point of view, is that its success in explaining phenomena does not depend on the ingenuity with which its contrivers 'save appearances' by introducing first one hypothetical force and then another. When the vortex-atom is once set in motion, all its properties are absolutely fixed and determined by the laws of motion of the primitive fluid, which are fully expressed in the fundamental equations. The disciple of Lucretius may cut and carve his solid atoms in the hope of getting them to combine into worlds; the follower of Boscovich may imagine new laws of force to meet the requirements of each new phenomenon; but he who dares to plant his feet in the path opened out by Helmholtz and Thomson has no such resources. His primitive fluid has no other properties than inertia, invariable density, and perfect mobility, and the method by which the motion of this fluid is to be traced is pure mathematical analysis. The difficulties of this method are enormous, but the glory of surmounting them would be unique" [p. 45].

Much misapprehension would seem to exist in regard to the physical side of the theory, especially in Germany,* where the mathematical investigations out of which it sprung, had their origin. Some appear to be unable to conceive how motion should take place in a material substance continuously filling space, losing sight of the fact that the liquid outside the atoms plays the part of a void (in so far as it cannot appeal to our senses)—or it is only the atoms that affect our perceptions. Others fail totally to appreciate the simplicity of the physical side of the theory, and seem to think it involves arbitrary postulates, whereas the main peculiarity of the theory is its freedom from positive assumptions, inasmuch as the theory evolves all the properties of matter out of the *motion* of a material substance, which without this motion has no positive qualities at all, and could not appeal to our senses. The fact seems to be overlooked that if we renounce the occult quality of *rigidity* in the atom, we have no other resource than a *liquid* (*i. e.*, a substance without rigidity). Much of the misunderstanding on the subject may no doubt be due to the scarcity of the literature relating to it, and the extreme brevity and absence of detail or attempt to assist the conceptions regarding the physical side of the theory. This want the author himself has much felt, and having been at considerable trouble to render clear his own conceptions as far as he could, he has thought that the result of this analysis might not perhaps be unacceptable in the form of a paper on the *physical aspects* of the theory.† For there are no doubt many investigators in the paths of natural science who may find some difficulty in realizing the physical basis and real bearings of the theory, and who nevertheless take a rational interest in the solution it is capable of affording to some of the greatest difficulties of molecular physics. The whole structure of physics may be said to rest upon a *molecular* basis, and therefore the im-

*The writer has had personal experience of this, partly through correspondence, and partly through the literature relating to the subject. Quotations from the writings of Prof. Zöllner especially seem to show a want of appreciation of the *physical* points of the theory at their true value and significance.

†As regards sources of information as to the vortex-atom theory, the following may be mentioned: Sir William Thomson, "On Vortex-Atoms," *Phila. Mag.*, July, 1867. Prof. Clerk-Maxwell, article "Atom," *Encyc. Brit.* 1875. The theory is dealt with to some extent in a popular manner in an article on "The Atomic Theory of Lucretius," *North British Review*, March, 1868, also by Prof. Tait, in his work "Lectures on Some Recent Advances in Physical Science."

portance of a right view of this basis cannot be over-estimated. The old theory of *perfectly rigid* molecules put an immense difficulty in the way of the development of physical results upon such a groundwork. A theory of *elastic* molecules therefore becomes of the utmost importance as a practical working hypothesis, and the accordance with observation of new results predicted from this hypothesis as a basis, will then form additional confirming illustrations of its truth. The removal of any misunderstandings that might be obstacles in the way of the use of the vortex-atom theory as a working hypothesis becomes, therefore, a point of considerable importance. Those more especially who have handled the spectroscope and viewed the exquisite precision of its results, become impressed with the *certainty* of the groundwork upon which their molecular studies are based, and no less imbued with the conviction of the existence of that *explanation* that forms the basis of the facts that are recorded with such unfailing accuracy.

REPORTS OF ENGINEERING SOCIETIES.

THE regular meeting of the ENGINEERS' CLUB OF PHILADELPHIA, was held on Saturday evening, May 15th, Mr. Frederic Graff, President, in the chair. The Committee on Improvement in Land Surveying in Pennsylvania, was announced as follows: Messrs. Chas. E. Billin, Chairman; Saml. L. Smedley, L. M. Haupt, W. C. Cranmer and John H. Dye. Mr. Arthur Sheafer read a paper on the Olean, Bradford and Warren and the Kendall and Eldred Railroads, in the oil regions of McKean Co , Pa. The O. B. & W. R. R. is 23 miles in length, from Bradford, Pa., to Olean, N. Y., reaching a height of 960 feet above Olean or 2398 feet above tide. Gauge, 3 feet; rails, 35 to 40 lbs. per yard; maximum grade, 185 feet per mile, two miles being at a grade of 185 feet per mile; maximum curve, 30 deg., 350 feet in length on trestle 25 feet high. The road was commenced in November, 1877, and in 60 days trains were running between the termini.

The K. & E. R. R. is 18¼ miles long from Bradford to Eldred, McKean Co. Gauge, weight of rails and maximum curves, same as O. B. & W.; maximum grade, 136 feet per mile; summit, 656 feet above Eldred or 2099 feet above tide. Crosses the Alleghany River on Howe truss bridge of two 90 feet spans. Its total cost, including equipment, was $150,000. In August, 1878, or 90 days after running preliminary lines, trains were running from Bradford to Eldred.

Mr. Neilson gave some notes on the Chicago & Tomah R. R. (narrow gauge), on which 20 lb. rails were used, even on 25 deg. curves, and trains of seven cars, each of 13 gross tons wt., were run.

Mr. A. R. Roberts announced a recent trial run on the Bound Brook R. R , by the single driver engine, of 89$\frac{3}{10}$ miles, in 97 minutes with four cars, and returning in 96 minutes with five cars. One run of 27 miles was made in 26¾ minutes. No heating of the machinery was observed. Mr. J. J. DeKinder illustrated the French method of sub-marine diving, which is a great improvement on the old method, with heavy helmets, etc. The apparatus is composed of a horizontal cylinder, surmounted by another cylinder at right angles to it, with a rubber cap. The lower cylinder is connected with the air pump by a tube, and the upper by another tube with the diver's mouth. A spring clamp is worn on the nose, the tube held in the mouth, and the apparatus worn on the back like a knapsack. By the action of valves, the air is circulated as the diver breathes, and he is encumbered with no other apparatus. His loaded shoes do not interfere with ease of motion, and he can rise at will. As little diving is done in winter, the temperature of the water is not an objection to its general use.

Mr. Freeland explained formulæ for a linkage connection for a valve motion. Mr. L. M. Haupt read an extract from a petition to Congress on river improvement.

The last meeting for the season, of the Engineers' Club of Philadelphia, was held on Saturday evening, June 5th, 1880—Mr. Percival Roberts, Jr., Vice President, in the chair—Mr. David Townsend read a paper on "A New Method for the Quantitative Determination of Combined Carbon in Cast Iron and Steel," and exhibited the apparatus for this purpose.

Mr. J. J. deKinder read a description of an Improved Apparatus for handling dredged material, designed by Mr. A. E. Hall, of Boston. By means of this apparatus dredged material can be conveyed from the dredging machine to the shore with equal facility at any stage of the tide, and without any intermediate handling of the material. The apparatus was illustrated by two large photographs.

Mr. Howard Murphy read, on behalf of Mr. J. Milton Titlow, a paper on "The Turn-table of Penrose Ferry Draw Span, Philadelphia."

The bridge that is swung by means of this turn-table is a through wrought-iron roadway bridge, 21 feet between centers of trusses and without footways.

The trusses are of the double cancel Pratt system, with inclined end posts and 411 feet between lower centers thereof, or about 415 feet over floor ; the depth at ends is 28 feet and 38 feet at center, the panel lengths being 15 feet except that at center which is 21 feet.

The four posts are equi-distant, transversely and longitudinally of bridge, and the center line of the drum passes through each of them, it being 30 feet in diameter and 6½ feet in height.

The turn-table is built so that it may be either rim or center bearing, but at present is used as the latter.

The weight upon the four center posts is carried to the center bearing by means of four

wrought-iron plate-girders 6 feet in depth and 30 feet long, which act as cantilevers and are placed side by side 3½ feet apart in two pairs, and at right angles; their ends being riveted to drum under posts of trusses. One pair of girders is set some three inches higher than the other, the plates of their top flanges being continuous across and through their intersection.

Within the box or space formed by the intersection of these girders stands the cone or pivot, the point of which is about on a level with their top flanges; above this the Sellers Box with 125 lineal inches of rolling and 56½ square inches of sliding surfaces, and upon this the carrying plate or table.

From this heavy plate the girders are suspended from their lower flanges by means of eight bolts, and by the nuts thereon the bridge may be raised or lowered to make the table either rim or center bearing.

Thus by simple construction with the same kind of material the weights are transferred as desired.

The live ring is formed of 51 wheels 16 inches in diameter and 7 inches tread.

The weight of the bridge is 300 tons, when closed and loaded 576 tons, weight of turntable, tracks, etc., 79 tons.

Upon the two segments of the turn-table outside of the trusses are placed on either side the Engine, Boiler, etc.

On account of the small space the engine stands parallel with the bridge, and the power is communicated by means of friction wheels and bevel gearing to two driving pinions on opposite sides of the rack, and to the two out end sets of screws, cams, etc, by means of which the ends are brought to bearings.

Mr. Rudolph Hering discussed the subject of the pollution of the Delaware and Schuylkill Rivers, and also, the intercepting sewers proposed by Mr. Darrach. From statistics covering the population living on the different drainage areas, the sewer connections and water closets, it is estimated that the sewerage of about 290,000 persons daily reaches the rivers, of which 167,500 drain into the Delaware, 119,500 into the Schuylkill below the dam and about 8,000, including the equivalent for the Manayunk Mills, into the river above the dam.

Comparing these quantities with the minimum flow of the two rivers after a long drought, it appears that at such times, the Delaware water will not be as wholesome as the Schuylkill, but that both are likely to be polluted above the admissable standard. How little use is made of the water carriage system may be seen from the fact that there are only 33,100 water closets in the city for 150,000 houses. It is estimated that nearly 500,000 persons make no use of the sewers, but use privy wells, which are periodically cleaned, but allow over 6 million cubic feet of fluid yearly to drain into the soil.

Intercepting sewers must soon carry this filth away from the city and its drinking water. Four-fifths of the drinking water is pumped from the Schuylkill and one-fifth from the Delaware.

Mr. Darrach's sewer to protect the Fairmount pool runs across the city into the Delaware, between the Kensington and Frankford pumps. It is also a mile longer than if it discharged below the Fairmount Dam, where no water is pumped.

Mr. Hering then described a system of intercepting sewers which he thought suited better our demands and was less expensive.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The May Number of the transactions contains the following papers:

No. 191—On the Variation Due to Orthogonal Strains in the Elastic Limits in Metals, by Robt H. Thurston.

No. 192—Experiments with Appliances for Testing Cement, by Alfred Noble.

No. 193—Design and Construction Table for Egg-Shaped Sewers, by C. G. Force, Jr.

No. 194—The Preservation of Timber, by J. W. Putnam.

IRON AND STEEL NOTES.

THE HISTORY AND MANUFACTURE OF STEEL.—Professor Alex. B. W. Kennedy, of University College, London, delivered last week two lectures on this subject at the Edinburgh Philosophical Institution. In the first lecture he spoke of the great change which had recently come about in the meaning of the word steel. For centuries, he said, steel had been a material of use chiefly for weapons, tools, and instruments where its extreme hardness and durability were its most valuable characteristics. But since 1830, when wrought iron first began to be used in large structures of any kind—ships, bridges, and so on—engineers had rather turned their attention to some of the other qualities possessed by steel, and had tried to find a material having the great strength of hard steel without its want of ductility. Such a material we now had in the so-called "mild steel" produced by the Bessemer and Siemens and Siemens-Martin processes—a material of enormous value in construction, but in reality often rather a pure iron than a steel proper. After a short description of some of the Eastern and other primitive methods of making steel, Professor Kennedy described in some detail the present method of making cast or crucible steel at Sheffield. He gave a short sketch of the life of Benjamin Huntsman, the Quaker inventor of the cast steel process in the early part of the last century, and of the ruse by which his brother steel makers succeeded in finding out his secret after their kindly attempt to prohibit the exportation of his steel—which they had at the same time declined to use themselves—had come to grief. He then sketched the various modifications of Huntsman's process now in use, described the leading characteristics of the materials produced by them, and concluded with a brief mention of some of the other steel-making processes, producing puddled steel, Uchatius steel, &c. In his second lecture he began by describing the common method of making wrought iron by "puddling," a process which he characterized as probably the roughest and most cruel of all metallurgical processes, whilst its rival—the Bessemer process—was the grandest and most beautiful. The processes of piling and rolling

bar iron were briefly described, in order to show the nature of the shortcomings in them, which were the causes of the great existing defects in wrought iron—defects of which the absence was essential to the development of the best properties of "mild steel" or ingot iron. The Bessemer process was then described in some detail, and the lecturer then went on to give an account of the "open hearth," or Siemens' process, as carried on at the Newton Works and elsewhere, mentioning some of its advantages, but declining to place the material produced by the one process higher than that made by the other. He then exhibited a number of specimens of mild steel, including samples of Sir James Whitworth's compressed steel, as well as of Bessemer steel, and some excellent forged work in Siemens steel made by Messrs. Denny, of Dumbarton. In reviewing the influence of the introduction of mild steel upon the iron industries, he paid special compliment to the Clyde shipbuilders for the way in which they had realized the advantages to be gained by the use of the new metal, and in which, through many difficulties, they had now come to reap in full success the reward. Not having for so long received their "fair share" (arithmetically speaking of Government work, they had been free to form and carry out their own ideas of what was best in design and in material, unhampered by the views of any Government department, relying solely on the excellence of the work they turned out, the trustworthiness of their steamers, and the economy of their engines. Speaking of the use of mild steel in bridges, the lecturer said he was sorry that under existing circumstances it was not possible to give any definite information as to the material to be used in the Forth Bridge, when and in whatever form it was ultimately decided to erect it. He hoped, however, that when constructed, it would be one of the greatest examples, if not the greatest, of the use of mild steel in the world, and would very probably be an example of a structure whose very existence would scarcely have been possible but for this material. In closing, the lecturer remarked that before another generation we should, perhaps, see the last of the "puddling" process; and besides having a finer material, we should have the satisfaction of having abolished forever one of the last remaining processes in which man had been used just as a strong brute—a process which had hitherto held its own against any attempts to improve it. He was sanguine that before long ingot iron might be used not only instead of wrought, but also instead of cast iron in very many circumstances.—*Engineering.*

THE STEEL TRADE OF THE WORLD.—The total capacity of the steel mills at the present time throughout the world is estimated at about 3,000,000 tons for the year's production. In the United Kingdom there are 120 Bessemer converters built, of which over 80 are at work, and the annual yield from these is considered at from 755,000 to 800,000 tons. The American make is estimated at 750,000 tons, the next largest producer being Germany, which is considered by many to be capable of the greatest expansion among all the steel-making countries. Less than two years ago there were 25 converters in Prussia working out of the 50 built, and turning out 375,000 tons; which were increased by the works in Saxony and the Palatinate to 400,000; and since the revival in trade fresh converters have been put into operation. The estimate of the French steel manufacture is about 275,000 tons; that of Belgium, 150,000; of Austria, with 32 converters, 250,000; and of Sweden and Russia, 150,000. Of the Bessemer converters in England, the largest are two ten ton ones at Sir John Brown and Co.'s works at Sheffield, the others varing between three and eight tons in capacity; and out of the 24 British steel works 17 only have rail mills. Looking at the probable extension of railways for the next twelve months, it is difficult to see how all this large output of steel rails is to be utilized.

RAILWAY NOTES.

IN offering prizes for the period of six years ending with July 15, 1881, the *German Railroad Union* suggests the following as especially desirable: (1) The invention of a locomotive, tender or car wheel of simple but safe design by which the loosening of tires will be effectively prevented. (2) The invention of a simple apparatus, which can be depended upon under all circumstances, which will render it possible for train men on different parts of a long train to communicate with the enginemen. (3) The invention of a cheap but reliable signal apparatus for the automatic blocking of trains which follow each other closely upon the open road, for regulating and rendering safe the traffic on crowded sections of road. (4) The invention of an apparatus which will make it possible for a train-man with the ordinary form of brake to apply the brakes simultaneously on two adjacent cars. This is required especially for freight cars. (5) Plans for improved statistics of the distribution and movement of cars, having regard to the administrative requirements of the separate roads, the settlement of the accounts for interchanged cars, and general statistical purposes. (6) The preparation of an exhaustive commentary on the working regulations, with special reference to the decisions of recent years. (7) A treatise based on statistical investigations on the influence and desirability of the present usual division of passengers and arrangement of cars into three or four classes, from a general public stand-point as well as with regard to the profit to the roads. (8) A short abridged encyclopædia of the technics of railroads, in the sense of genuine encyclopædia; that is, a systematic grouping of the materials and their relation to each. (9) A history of the development of freight tariffs and their influence on the public welware.—*Engineer.*

A LETTER from Naples, written by one of the nine persons who made the experimental trip on the new railway to the crater of Vesuvius, gives some particulars of the line and the journey. The actual railroad is 800 meters long and terminates 200 meters short of the

mouth of the crater. The inclines are tremendous: Four in 10 for the first 135 meters; 63 in 100 for the next 330 meters; then 56, 52, and finally 48 in the 100, for the remainder. The carriages are drawn up by a steel rope of forty nine strands, which is coated with tar as a protection against rust. An hour's drive from Naples takes the traveler to the mountain observatory. An excellent new road, nearly two miles long, has been built by the railway company from the observatory to the railway station. The ascent on the railway was made in seven minutes, but it can easily be made in five. The motion was quite smooth, but the sensation on looking out is far from pleasant, and a feeling akin to seasickness is said to arise. The view from the summit repays all the trouble. The writer says that at every step one feels the proximity of the great storehouse of heat. He was informed that great pillars of smoke frequently burst up from the ground, close to the spot where the railroad ends, and great chasms open, swallowing up anything which may be on the spot, so that the expedition may sometimes not be wholly free from danger. It was intended to open the line for the public at the beginning of May.

ENGINEERING STRUCTURES.

THE TAY BRIDGE DISASTER.—After a protracted inquiry, extending over several weeks and involving twenty-six sittings, the Commissioners appointed to investigate the Tay Bridge disaster, with the view of determining its cause, have adjourned their meetings *sine die*. Those of our readers who have followed our weekly reports of the proceedings—condensed though they necessarily have been—or the more detailed statements in the daily papers, cannot fail to have observed that a vast amount of evidence was given, and that a large proportion of that evidence was of a very conflicting and contradictory nature. The contradictions, however, were largely confined to matters of opinion. Matters of fact could scarcely be liable to contradiction, and these to a great extent went to show the existence of defects in the parts of the structure. It may probably be some little time before the report of the Commissioners is made public, and until that time arrives it would be highly indecorous on our part to offer any critical observations on the evidence taken, or to indicate the conclusions to which it points. Indeed, this latter course would be somewhat difficult, owing to the conflicting nature of some of the statements, as already mentioned, and which may possibly necessitate further and personal local inquiry and investigation on the part of the Commissioners before their report can be completed. Originally commenced in Dundee and continued in London, the inquiry may possibly terminate in the former town. It may be taken for granted that the investigations of the Commissioners will be of the most searching character, their object being to render the repetition of such an accident as that of the 28th December last, as far as they can, impossible, by bringing to light its causes.

It is well known that the bridge, up to the time of its destruction, was generally looked upon as a pattern structure of its kind and one well worthy of imitation elsewhere as occasion might require. It is no less well known that not only structurally was the bridge thus viewed, but as a model of cheapness and rapid construction. As we briefly wrote on the 2nd of January last, so we now repeat, that it is impossible not to believe that those who had charge of the designing and constructing of the bridge did their best to ensure its safety. They doubtless took into consideration all the contingencies that were ever likely to affect its strength and stability; and it is in evidence that they prepared their designs accordingly, and in accordance with the best principles of modern engineering construction. The evidence of Mr. Benjamin Baker, a gentleman well qualified to offer an opinion, goes to prove this very strongly. He moreover stated that he considered the workmanship of the Tay Bridge, generally, was of good character. He observed, however, that good and bad work were relative and that he had seen both better and worse work than there was in that structure. But, however correct may have been the design, and however sound the general execution, there still remains the fact that the bridge was a marvel of cheapness and of rapid construction; and it may be a question how far these two conditions have affected and influenced the character of the more minute details of the work. At the same time and in the face of the defects which are stated to have existed in some of the castings, it is in evidence that those who were responsible appeared to have been fully alive to their responsibilities and to have acted accordingly.

Then again we have it stated that inferior Cleveland iron was used in some portions of the structure, against which we have the Commissioners stating that the court had received the results of some tests made by Mr. Kirkaldy as to the quality of the iron, and that the opinion of the court was that the iron was exceptionally good. It appears that the wrought iron in the bolts was of excellent quality, and only broke under a strain of 25 tons per square inch, whilst the tie-bars did not give way in the eyes until a strain of 20 tons per square inch had been reached. Portions of the girders bore respectively strains of 22¼ and 23¼ tons per square inch before breaking, and strips cut from the broken cast-iron columns bore a direct tensile strain of 9¼ tons per square inch before yielding to stress. These figures speak for themselves and for the character of the metal. Mr. Law's evidence is certainly very damaging. He reported fully on the whole construction, which he condemns *in toto*, leaving an impression not of the most pleasant nature, and one, moreover, which it is to be hoped the report of the Commissioners will tend to modify. Then as to the Government inspection of the bridge, it is stated that that was far more carefully and closely performed than usual, and the tests showed the bridge to be far more stiff than had been anticipated. Thus, from first to last, the broad and general conditions of safety appear to have been duly re-

garded. It, however, remains to be seen to what extent subordinate supervision was carried, and whether sufficient attention was paid to the multifarious requirements of the structure in detail. There can be no doubt whatever that all these points will receive the fullest consideration at the hands of the Commissioners, and although the inquiry cannot recall the past, it can, and doubtless will, prove profitable in the future.

ORDNANCE AND NAVAL.

THE fragments of the 38-ton gun destroyed for experimental purposes in the bursting-cell in the proof-grounds, Government Marshes, adjoining the Royal Arsenal, Woolwich, on Tuesday last, have all been recovered, and are found to number about 120 pieces. They have all been marked, and are being washed and arranged for inspection. The two projectiles were taken from the sand butt in front of the gun, both broken in pieces, and it is evident from the appearance of the bore that they broke up before leaving the gun, the marks of the rifling being in parts quite effaced. The muzzle end of the steel tube, about 3 feet in length, is intact, with parts of the wrought iron super-coil remaining attached, and a singular appearance is presented by the rearmost end of this fragment, the steel having been violently rent and incurved as though a shot or lighter fragment, moving faster than itself, had overtaken it and struck it with considerable force. The crusher gauges fixed on both projectiles have been recovered, but give no positive data respecting the pressure produced by the explosion. A very great pressure had been expected, and the copper crushers had consequently been subjected to a pressure of thirty-five tons to the square inch before being inserted in the plugs. This pressure was not exceeded in the explosion, and the only apparent deduction arrived at of importance is that a strain which would not be alarming in the powder chamber has sufficed to burst the gun at the spot where its thickness and strength suddenly diminished.

THE RECOIL OF ORDNANCE.—One of the greatest difficulties attending the introduction of improved gunpowders and the consequent increase of power imparted to the guns has been the correspondent development of greater recoil. This has been a source of inconvenience in relation to all kinds of ordnance, but it has been increasingly felt in dealing with the lighter descriptions of guns, such, for instance, as those of the horse and field artillery and the siege train. With naval guns and garrison artillery the weight of the equipment, aided by breaks and hydraulic buffers, has to some extent met the difficulty, and improvements are now being tried at the Royal Arsenal, Woolwich, with contrivances by which it is hoped that at least a portion of the recoil may be absorbed even in the lightest of the gun carriages. A 64-pounder gun carriage is being experimentally fitted with hydraulic buffers, which will, undoubtedly, receive a portion of the shock on discharge, but as the recoil necessarily increases in ratio to the energy imparted to the projectile it is far from certain that the device will meet the growing demands of the artillerists.

A NEW explosive, denominated potentite, is finding favor for blasting purposes in the Cumberland and Furness mines.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

We are indebted to Mr. James Forrest for the following publications of THE INSTITUTION OF CIVIL ENGINEERS:

"Dredging Operations on the Danube." By Murray Jackson.

"The Thames Steam Ferry between Wapping and Rotherhithe." By Frederick Eliot Duckharn, M. I. C. E.

"The River Nile." By Benjamin Baker, M. I. C. E.

"New Zealand Lighthouses." By John Blockitt, M. I. C. E.

"Fire Hydrant." By Edward Henry Keating, M. I. C. E.

"A Rack-Railway Worked by Endless Ropes." By T. Aguido.

"Tunnel Outlets from Storage Reservoirs." By Charles John Wood, M. I. C. E.

"The Theory of Modern American Suspension Bridges." By Prof. Celeste Clericetti.

THE IRON, STEEL, AND ALLIED TRADES IN 1879. ANNUAL REPORT TO THE MEMBERS OF THE BRITISH IRON TRADE ASSOCIATION. London: E. & F. N. Spon, and British Iron Trade Association. For sale by D. Van Nostrand.

The report before us comprises within its 111 pages a great variety of valuable statistics of especial interest at the present time to all concerned in the prosperity of our iron and steel industries, and it altogether reflects great credit upon Mr. Jeans, the secretary of the British Iron Trade Association (and also of the Iron and Steel Institute) who is responsible for its compilation.

The contents are divided into eleven chapters, of which the first deals with production and importation of iron ores.

Chapter II. deals with the pig iron trade, and contains copious information respecting the production of different districts, quantities of stocks, shipments, and prices of pig iron during series of years, together with notes concerning blast furnaces and the consumption of coal per ton of iron made. Chapter III. deals in a similar way with the manufactured iron trade, the information given being very varied.

The fourth chapter treats of the Bessemer steel trade, and from it we learn that at the end of 1879 there were 66 Bessemer converters in use in this country, while 38 were idle, and 11 in course of erection. The total quantity of steel ingots produced by the Bessemer process

in 1879 was 834,711 tons, while the quantity of steel rails turned out was 520,231 tons.

The next chapter deals with British exports and imports of iron and steel, and from it we learn that the exports of 1879 showed an increase in quantity of 583,000 tons, and an increase of value of £1,045,000 as compared with the previous year, the quantity exported being in fact greater than during any year since 1873.

Chapter VII. treats of the coal trade in 1879, and shows amongst numerous other facts that the exports last year reached 16,535,642 tons as compared with 15,494,633 tons in 1878. Chapter VIII. treats of shipbuilding, and comprises some specially interesting statistics relating to the use of steel for this purpose, while next comes a chapter on railways and the iron trade, containing a variety of interesting information. "The Foreign Iron and Coal Trades in 1879" forms the subject of Chapter X., this chapter containing statistics which render possible some interesting comparisons between our own progress and that of other nations. Finally we have a chapter on "Tariff Legislation and the British Iron Trade," in which the tariff legislation of the last twenty years is reviewed, and information given as to the present aspect of the subject in Germany, France, Canada, and the United States. Altogether, as we have said, the report before us is a very creditable one to all concerned in its production, and we have no doubt that the information it affords will be widely appreciated.

MATHEMATICAL DRAWING INSTRUMENTS, AND HOW TO USE THEM. BY F. EDWARD HULME, F. L. S. London: Trübner & Co. For sale by D. Van Nostrand. Price, $1.50.

This brief treatise is a convenience to the student, and invaluable to any one who is compelled to acquire a knowledge of draughting without a teacher. The illustrations are exceedingly good, and the instruction is throughout explicit.

GEOLOGY FOR STUDENTS AND GENERAL READERS. BY H. A. GREENE, M. A.; F.G.S. London: Rivingtons. For sale by D. Van Nostrand. Price, $5.00

For a general book of reference relating to the technical points of descriptive and dynamical geology, nothing could be better, apparently, than this work. This remark seems necessary in view of the fact that geology for general reading is often held to imply essays like "Testimony of the Rocks," "Old Red Sandstone," etc.

The scope of the book before us may be inferred from the list of subjects:

Chapter 1. The Aim and Scope of Geology, with a sketch of its rise and progress.
Chapter 2. Descriptive Geology.
Chapter 3. Denudation.
Chapter 4. What becomes of the waste produced and carried off by denudation? The Method of Formation of Bedded Rocks, and some Structures impressed on them after their Formation.
Chapter 5. Definition and Classification of Derivative Rocks and how, from a study of their character, we can determine the physical geography of the earth at different periods of its past history.
Chapter 6. Volcanic Rocks.
Chapter 7. Metamorphic Rocks.
Chapter 8. Granite.
Chapter 9. How the Rocks came into the Positions in which we find them.
Chapter. 10. How the present surface of the Ground has been produced.
Chapter 11. Original Fluidity and Present Condition of the Interior of the Earth. Cause of Upheaval and Contortion. Origin of the Heat Required for Volcanic Energy and Metamorphism. Remarks on Speculative Geology.
Chapter 12. On Changes of Climate, and how they have been brought about.

THE ART OF PERFUMERY. BY G. W. SEPTIMUS PIESSE, Ph. D., F. C. S. Fourth Edition. Philadelphia: Presley Blakiston. For sale by D. Van Nostrand. Price, $5.50.

This work relates to a branch of industry which directly interests a large number of workers, and indirectly the public at large. It is essentially practical in its character, and is designed for dealers and manufacturers. It is beautifully illustrated, and includes an appendix on the artificial fruit essences for confectionary and syrups.

TURNING AND MECHANICAL MANIPULATION. Vol. IV. PRINCIPLES AND PRACTICE OF HAND OR SIMPLE TURNING. BY JOHN JACOB HOLZAPFFEL. London: Holzapffel & Co. For sale by D. Van Nostrand. Price, $10.00.

This volume, which is of octavo size, is devoted exclusively to lathe work. Everything which the lathe is supposed capable of doing is discussed and illustrated in this book. The subjects treated by chapters are:

1. Introductory. Early History.
2. Center Lathes. Continuous Motion.
3. Lathes with Revolving Mandreis.
4. Modern Foot-Lathes.
5. Apparatus for Special Purposes.
6. Chucks and Apparatus for holding.
7. Practice of Soft-wood Turning.
8. Practice of Hard-wood and Ivory Turning.
9. Elementary Metal Turning.
10. Screw Cutting.
11. The Sphere, and forms derived from this solid.
12. Examples of Simple Plain Turning.
13. Examples of Combined Plain Turning.
14. Miscellanea. Staining, Dyeing, etc.

MISCELLANEOUS

A DEEP BORE HOLE.—The Continental Diamond Rock-Boring Company, Limited, have lately completed for the Government of Mecklenburg-Schwerin a bore-hole of exceptional depth, and the execution of which is of particular interest from the rapidity with which it has been completed. The boring, which was made for salt, is situated at Probst Jesar, near Lubtheen, and it was commenced on the 6th of July of last year, with an opening 12 inches in diameter. The first part of the bore had to be through a diluvial bed consisting mainly of drift sand and coarse gravel, and for sinking through this Kobrich's system was adopted, the diameter of the bore being maintained at 12 inches. The total depth sunk on this system was 98.05 meters, or 321 feet 8 inches, the sinking occupying 34 days of 24 hours each, of which 31 days were spent in actual boring and three days in sundry works. The average progress was thus at the rate of 3.163 meters per day, while the greatest depth bored in one day was 7.496 meters, this being on August 11, 1879.

Below the diluvium the gypsum and rock

were reached, and through this the boring was carried on with diamonds, the commencement being made on August 25, 1879, with a hole 10½ inches in diameter. Until a depth of 509 meters, or 1670 feet, had been reached, however, no firm footing could be obtained on which to rest the tubing, and hence great annoyance was experienced from the falling in of masses of sand, the infalls being so great that sometimes when the boring rod was withdrawn the bore became filled up again to a depth of over 420 feet. The boring, however, was steadily proceeded with, and ultimately the final depth of 1207.25 meters, or 3961 feet was attained on the 6th of February last, the diameter of the bore at the bottom being 3 inches. The time spent in boring with diamonds was thus 163 days of 24 working hours, and this time was accounted for as follows:

	days.
For progressive boring	70
" reaming up	8
" sundry works, *i. e.*:	
Getting rid of infall..... 13	
Preparing and repairing tools.. 42	
Preparing lye..... 6	.. 85
Letting down tubing..... 22	
Making good an accident..... 2	
Total	163

Altogether the depth reamed up amounted to 299.205 meters, the time occupied being divided as follows:

	meters.		in.		in.		days.
Enlarging	10.95	from	9	to	10½	in diameter.	0.5
"	87.555	"	8	"	9	"	.. 3.0
"	98.000	"	7	"	8	"	.. 1.0
"	39.700	"	3	"	5	"	.. 0.5
"	63.000	"	3	"	4	"	.. 3.0

The greatest progress made in any one day was on the 27th of January last, when a depth of 29 meters (95 feet 2 inches) was bored, this being nearly double the average progress The total length of tube inserted was 1010.55 inches, or 3315½ feet, the greatest length inserted in one piece being 456.424 meters, or 1497½ feet and this consisting of 7 inch and 8 inch tubes. Throughout the whole depth of the bore cores were drawn, some of these being salt cores over 2 feet long in one piece.

With the exception of a bore-hole put down to the depth of 1275 meters, or 4183 feet, for the Prussian Government a few years ago, and which took four years to accomplish, the bore of which we have been giving particulars is, we believe, the deepest yet sunk, and the fact that it was completed in less than six months speaks well for the skill and energy with which the work was carried out.

M. COLLADON has read a paper "On the Meeting of the Two Advance Galleries of the Great St. Gothard Tunnel," which gives various interesting details, including the volume of infiltrations in the south gallery which reached 230 liters per second. The difference of level at meeting was not over 0.10m.; the lateral deviation less than 0.20m. The total length measured in the tunnel was nearly 8m. less than that calculated geometrically. The official statement made by the Swiss Federal Council shows that the cost of the St. Gothard Tunnel from the commencement up to March 1st., the total amount expended on the work was 45,600,000f., or £1,824,000 sterling. The work on the Airolo side cost rather less than on the Göschenen side, the amounts expended being 21,800,000f. and 23,200,000f. respectively. The difference about corresponds with the different lengths done on the two sides, the portion from the Göschenen end being rather more than half. The finishing operations will take some little time, and it is estimated that by the time the tunnel is ready to be handed over for traffic it will have cost altogether about 50,000,000f., or £2,000,000 sterling. This will bring the cost up to about 1000f., or £40 per foot.

ACCORDING to the last report of the Indo-European Telegraph Department, the distance between London and Teheran is about 3,800 miles; the average time of transit of all messages is given as 17 min. 30 sec.; while the time occupied in transmitting messages between Teheran and Bushire is stated at 2 min. 58 sec.

TWO German inventors, Breuer and Schumacher, have made a new form of machine for separating the turnings and borings of brass and copper from those of iron and steel The mixed metals fall on a magnetised cylinder or drum, to which the iron and steel adhere, while the copper and brass fall into a special reservoir below. There are two hollow cylinders rotating in the same direction, so that the iron which escapes from the first cylinder is retained by the second. The surface of the cylinder is formed by flat bands or strips of soft iron alternating with strips of copper, and each of the iron bands is in contact with a row of horseshoe magnets. The adherent metal is removed by revolving brushes.

AT the foot of Mt. Vesuvius there is now the new station of the railway to the summit of the old crater. It is on a level spot on the west side of the mountain, about half-an-hour's walk from the Observatory. As before, the traveler must reach the Observitory from Resina by carriage or on horseback. There are two lines of rails, each provided with a carriage divided into two compartments and capable of holding six persons. While one carriage goes up the other comes down, the two hanging from the end of a wire rope running over a pulley at the summit. The incline is very steep, commencing at 40 deg., increasing to 63 deg. and continuing at 50 deg. to the summit. The ascent will be made in eight to ten minutes, which before required from one to two hours. To obtain the necessary supply of water, large covered cisterns have been constructed, which in winter will be filled with the snow that often falls heavily on Vesuvius. On reaching the top there is still the new and smaller cone to be ascended by those who are charmed by risk and adventure.